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Contents

SCIENCE AND ENGINEERING

Pure Science, Applied Science, Technology, Engineering: An Attempt at Definitions.....*James K. Feibleman* 305

Engineering and Science: A Historical Review and Appraisal *James Kip Finch* 318

Engineering and the Scientific Revolution.....*A. Rupert Hall* 333

The Technological Revolution: Notes on the Relationship of Technology, Science, and Culture.....*Peter F. Drucker* 342

Chemistry, Chemical Technology, and Scientific Progress.... *Henry M. Leicester* 352

The Interaction of Science and Practice in the History of Metallurgy.....*Cyril Stanley Smith* 357

Science and Engineering in Agriculture: A Historical Perspective.....*Fred W. Kohlmeyer and Floyd L. Herum* 368

Science and the Steam Engine.....*Milton Kerker* 381

Science and Engineering in the History of Aviation..... *John B. Rae* 391

BOOK REVIEWS

The Construction of Gothic Cathedrals: A Study of Medieval Vault Erection by John Fitchen.....*L. R. Shelby* 400

American Building Art: The Nineteenth Century by Carl W. Condit .....*Harry Mahler* 402

France and the Economic Development of Europe, 1800-1914 by Rondo E. Cameron.....*Warren C. Scoville* 403

The Union Pacific Railroad: A Case in Premature Enterprise by Robert William Fogel.....*Robert E. Carlson* 405
Vanishing Crafts and Their Craftsmen by Rollin G. Steinmetz
Kendall Birr 407
Black Rock: Mining Folklore of the Pennsylvania Dutch by George Korson.....*Charles O. Houston, Jr.* 408
Steelworkers in America: The Nonunion Era by David Brody
Jeanne McHugh 409
Blue Collar Man: Patterns of Dual Allegiance in Industry by Theodore V. Purcell .....*Lawrence R. Chenault* 411
Armor: A History of Mechanized Forces by Richard M. Ogorkiewicz .....*I. B. Holley, Jr.* 415
U. S. Army in World War II. The Technical Services. The Chemical Warfare Service: Organizing for War by Leo P. Brophy and George J. B. Fisher. *From Laboratory to Field* by Leo P. Brophy, Wyndham D. Miles, and Rexmond C. Cochrane .....*Herman Skolnik* 417
Elihu Thomson: Belived Scientist by David O. Woodbury
Thomas P. Hughes 419
The Modernization of Iran, 1921-41 by Amin Banani.....
David Rodnick 420
A Nationalized Industry and Public Ownership by William A. Robson .....*Edward Wisnewsky* 422
Central Planning in Czechoslovakia: Organization for Growth in a Mature Economy by Jan M. Michal..*Edmund M. Bleich* 426
The Crisis We Face: Automation and the Cold War by George Steele and Paul Kircher.....*A. J. Jaffe* 428
Leisure in America: A Social Inquiry by Max Kaplan.....
Norman D. Kurland 431
Psychophysiological Aspects of Space Flight by Bernard E. Flaherty .....*Eric T. Carlson* 433

ORGANIZATIONAL NOTES 435

ANNOUNCEMENTS 437

INDEX 440

Pure Science, Applied Science, Technology, Engineering: An Attempt at Definitions

JAMES K. FEIBLEMAN*

1. *The Theory of Practice*

IT IS NOT the business of scientists to investigate just what the business of science is. Yet the business of science is in need of investigation. If we are to consider the relations between science and engineering, the relation between pure and applied science will have first to be made very clear; and for this purpose we shall need working definitions. Once stated, these definitions may seem an elaboration of the obvious and an oversimplification. But the elaboration often seems obvious only *after* it has been stated, and the definitions may have to be simple in order to bring out the necessary distinctions.

By "pure science" or "basic research" is meant a method of investigating nature by the experimental method in an attempt to satisfy the need to know. Many activities in pure science are not experimental, as, for instance, biological taxonomy; but it can always be shown that in such cases the activities are ancillary to experiment. In the case of biological taxonomy the classifications are of experimental material. Taxonomy is practiced in other areas where it is not scientific, such as in the operation of libraries.

By "applied science" is meant the use of pure science for some practical human purpose.

Thus science serves two human purposes: to know and to do. The former is a matter of understanding, the latter a matter of action. Technology, which began as the attempt to satisfy a practical need without the use of science, will receive a fuller treatment in a later section.

Applied science, then, is simply pure science applied. But scientific method has more than one end; it leads to explanation and application. It achieves explanation in the discovery of laws, and the laws can be applied. Thus both pure science and applied science have aims and results. Pure science has as its aim the understanding of nature; it

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seeks explanation. Applied science has as its aim the control of nature; it has the task of employing the findings of pure science to get practical tasks done. Pure science has as a result the furnishing of laws for application in applied science. And, as we shall learn later in this essay, applied science has as a result the stimulation of discovery in pure science.

Applied science puts to practical human uses the discoveries made in pure science. Whether there would be such a thing as pure science alone is hard to say; there are reasons for thinking that there would be, for pure science has a long history and, as we have noted, another justification. There could be technology without science; for millenia, in fact, there was. But surely there could be no applied science without pure science: applied science means just what it says, namely, the application of science, and so without pure science there would be nothing to apply.

Logically, pure science pursued in disregard of applied science seems to be the *sine qua non* of applied science, while historically the problems toward which applied science is directed came before pure science.

It has been asserted, for instance, that Greek geometry, which is certainly pure, arose out of the interest in land surveying problems in Egypt, where the annual overflow of the Nile obliterated all conventional boundaries. Certainly it is true that the same concept of infinity is necessary for the understanding of Euclidean geometry and for the division of farms. Be that as it may, it yet remains true that the relations between pure and applied science are often varied and subtle, and will require exploration.

Let us propose the hypothesis that all pure science is applicable.

No proof exists for such an hypothesis; all that can be offered is evidence in favor of it. This evidence consists of two parts, the first logical and the second historical.

The logical evidence in favor of the hypothesis is contained in the very nature of pure science itself. Any discovery in pure science that gets itself established will have gained the support of experimental data. Thus there must be a connection between the world of fact, the actual world, in other words, which corresponds to sense experience, and the laws of pure science. It is not too difficult to take the next step, and so to suppose that the laws, which were suggested by facts in the world corresponding to sense experience, could be applied back to that world.

The second part of the evidence for the applicability of all of the laws of pure science is contained in the record of those laws which have been applied. The modern western cultures have been altered

by applied science, and now Soviet Russia and China are following their lead. Indeed, so prevalent are the effects of applied science, and so concealed the leadership of pure science, that those whose understanding of science is limited are apt to identify all of science with applied science and even to assume that science itself means technology or heavy industry.

One argument against the position advocated here would be based upon the number of pure theories in science for which no application has yet been found. But this is no argument at all; for to have any weight, it would have to show not only that there had been no application but also why there could be none. Yet a time lag between the discovery of a theory and its application to practice is not uncommon. How many years elapsed between Faraday's discovery of the dynamo and its general manufacture and use in industry? Conic sections were discovered by Apollonius of Perga in the third century B. C., when they were of intellectual interest only, and they were applied to the problems of engineering only in the seventeenth century. Non-Euclidean geometry, worked out by Riemann as an essay in pure mathematics early in the nineteenth century, was used by Einstein in his theory of relativity in the twentieth century. The coordinate geometry of Descartes made possible the study of curves by means of quadratic equations. It was in Descartes' time that the application of conic sections to the orbit of planets was first noticed; later the same curves were used in the analysis of the paths of projectiles, in searchlight reflectors, and in the cables of suspension bridges. Chlorinated diphenylethane was synthesized in 1874. Its value as insecticide (DDT) was not recognized until 1939 when a systematic search for moth repellants was undertaken for the military. The photoelectric cell was used in pure science, notably by George E. Hale on observations of the sun's corona in 1894. Twenty-five years later it was found employed in making motion pictures. It often happens that the discovery of a useful material is not sufficient; it is necessary also to discover a use, to connect the material to some function in which it could prove advantageous. Paracelsus discovered ether and even observed its anaesthetic properties, and Valerius Cordus gave the formula for its preparation as early as the sixteenth century. But it was many centuries before ether was used as an anesthetic.

Presumably, then, pure scientific formulations which have not been applied are merely those for which as yet no applications have been found. In the effort to extend knowledge it is not strategically wise to hamper investigation with antecedent assurances of utility. Many of the scientific discoveries which later proved most advantageous in

industry had not been self-evidently applicable. This is certainly true of Gibbs' phase rule in chemistry, for instance. It often happens that for the most abstract theories new acts of discovery are necessary in order to put them to practical use.

2. *From Theory to Practice*

It should be observed at the outset that applications are matters of relevance. The line between pure and applied science is a thin one; they are distinct in their differences, but one fades into the other. For instance, the use of crystallography in the packing industry is an application, but so is the use of the mathematical theory of groups in pure crystallography and in quantum mechanics. The employment of mathematics in pure science means application from the point of view of mathematics but remoteness from application from the point of view of experiment. Some branches of mathematics have been so widely employed that we have come to think of them as practical affairs. This is the case with probability or differential equations. Both branches, however, considered in their mathematical aspects and not at all in relation to the various experimental sciences in which they have proved so useful, are theoretical disciplines with a status of their own which in no wise depends upon the uses to which they may be put.

Procedurally, the practitioner introduces into his problem the facilitation afforded by some abstract but relevant theory from either mathematics or pure science. The statistical theory of extreme values is a branch of mathematics, yet it has application to studies of metal fatigue and to such meteorological phenomena as annual droughts, atmospheric pressure and temperature, snowfalls, and rainfalls. The discovery of the Salk vaccine against polio virus was an achievement of applied science. Yet Pasteur's principle of pure science, that dead or attenuated organisms could induce the production of antibodies within blood serum, was assumed by Salk; so the immensely important practical applications would not have been possible without the previous theoretical work.

It is clear, then that we need three separate and distinct kinds of pursuits, and, perforce, three types of interest to accompany them. The first is pure science. Pure theoretical sciences are concerned with the discovery of natural law and the description of nature, and with nothing else. These sciences are conducted by men whose chief desire is to know, and this requires a detached inquiry—which Einstein has somewhere called " the holy curiosity of inquiry " and which Emerson declared to be perpetual. Such a detachment and such a pursuit are

comparable in their high seriousness of purpose only to religion and art.

The second type is applied science, in which are included all applications of the experimental pure sciences. These are concerned with the improvement of human means and ends and with nothing else. They are conducted by men whose chief desires are practical: either the improvement of human conditions or profit, or both. Temperamentally, the applied scientists are not the same as the pure scientists: their sights while valid are lower; they are apt to be men of greater skill but of lesser imagination; what they lack in loftiness they gain in humanity. It would be a poor view which in all respects held either variety secondary. Yet there is a scale of order to human enterprises, even when we are sure we could dispense with none of them; and so we turn with some measure of dependence to the type of leadership which a preoccupation with detached inquiry is able to provide.

The third type is the intermediate or *modus operandi* level, which is represented by the scientist with an interest in the solution of the problems presented by the task of getting from theory to practice. As Whitehead said, a short but concentrated interval for the development of imaginative design lies between them. Consider for example the role of the discovery of Hertzian waves, which not only led to the development of radio but also brilliantly confirmed Maxwell's model of an electromagnetic field, specifically the existence of electromagnetic waves, with a constant representing the velocity of light in two of the four equations.

The conception of science as exclusively pure or utterly applied is erroneous; the situation is no longer so absolute. When scientific theories were not too abstract, it was possible for practical-minded men to address themselves both to a knowledge of the theory and to the business of applying it in practice. The nineteenth century saw the rise of the "inventor," the technologist who employed the results of the theoretical scientist in the discovery of devices, or instruments, of new techniques in electromagnetics, in chemistry, and in many other fields. Earlier scientists like Maxwell had prepared the way for inventors like Edison. In some sciences, notably physics, however, this simple situation no longer prevails. The theories discovered there are of such a degree of mathematical abstraction that an intermediate type of interest and activity is now required. The theories which are discovered in the physicists' laboratories and published as journal articles take some time to make their way into engineering handbooks and contract practices. Some intermediate theory is necessary for getting from theory to practice.

A good example of the *modus operandi* level is furnished by the activities and the scientists concerned with making the first atomic bombs. Hahn and Strasmann discovered in 1938 that neutrons could split the nuclei of uranium. Einstein and Planck had earlier produced the requisite theories, but it was Enrico Fermi, Lise Meitner, and others who worked out the method of getting from relativity and quantum mechanics to bombs which could be made to explode by atomic fission.

3. *Technology*

There has been some misunderstanding of the distinction between applied science and technology; and understandably so, for the terms have not been clearly distinguished. Primarily the difference is one of type of approach. The applied scientist as such is concerned with the task of discovering applications for pure theory. The technologist has a problem which lies a little nearer to practice. Both applied scientist and technologist employ experiment; but in the former case guided by hypotheses deduced from theory, while in the latter case employing trial-and-error or skilled approaches derived from concrete experience. The theoretical biochemist is a pure scientist, working for the most part with the carbon compounds. The biochemist is an applied scientist when he explores the physiological effects of some new drug, perhaps trying it out to begin with on laboratory animals, then perhaps on himself or on volunteers from his laboratory or from the charity ward of some hospital. The doctor or practicing physician is a technologist when he prescribes it for some of his patients.

Speaking historically, the achievements of technology are those which developed without benefit of science; they arose empirically either by accident or as a matter of common experience. The use of certain biochemicals in the practice of medicine antedates the development of science: notably, ephedrine, cocaine, curare, and quinine. This is true also of the pre-scientific forms of certain industrial processes, such as cheese-making, fermentation, and tanning.

The applied scientist fits a case under a class; the technologist takes it from there and works it out, so to speak, *in situ*. Applied science consists in a system of concrete interpretations of scientific propositions directed to some end useful for human life. Technology might now be described as a further step in applied science by means of the improvement of instruments. In this last sense, technology has always been with us; it was vastly accelerated in efficiency by having been brought under applied science as a branch.

Technology is more apt to develop empirical laws than theoretical

laws, laws which are generalizations from practice rather than laws which are intuited and then applied to practice. Empirical procedures like empirical laws are often the product of technological practice without benefit of theory. Since 1938, when Cerletti and Bini began to use electrically induced convulsions in the treatment of schizophrenia, the technique of electroshock therapy has been widespread in psychiatric practice. Yet there is no agreement as to what precisely occurs or how the improvement is produced; a theory to explain the practice is entirely wanting.

Like applied science, technology has its ideals. Let us consider the technological problem of improving the airplane, for instance. For a number of decades now, the problem to which airplane designers have addressed themselves is how to increase the speed and the pay load of airplanes. This means cutting down on the weight of the empty airplane in proportion to its carrying capacity while increasing its effective speed. If we look back at what has been accomplished in this direction, then extrapolate our findings into the future in order to discern the outlines of the ideal, we shall be surprised to discover that what the designers have been working toward is an airplane that will carry an infinite amount of pay load at an infinite speed while itself weighing nothing at all! This of course is a limit, and like all such limits, is an ideal intended to be increasingly approached without ever being absolutely reached.

Conception of the ideal is evidently of the utmost practicality and cannot be escaped in applied formulations. Yet the existence of such a thing as a technical ideal is fairly recent and is peculiar to western culture. The ideal of a general character envisaged in this connection is that of fitness of purpose and of economy; no material or energy is to be wasted. Roman engineers built bridges designed to support loads far in excess of anything that might be carried over them; their procedures would be regarded as bad engineering today. The modern engineer builds his bridge to carry exactly the load that will be put upon it plus a small margin for safety, but no more; he must not waste structural steel nor use more rivets than necessary, and labor must be held to minimum. The ideal of technology is efficiency.

Although technologists work in terms of ideals, they are nevertheless more bound down to materials than is the applied scientist, just as the applied scientist in his turn is more bound down to materials than is the pure scientist. Since the technologist is limited by what is available, when he increases the going availability it is usually at the material level. The environment with which a society reacts is the available environment, not the entire or total environment; and the

available environment is that part of the environment which is placed within reach of the society by its knowledge and techniques. These are laid out for it and increased by the pure and the applied sciences. Only when these limits are set can the technologist go to work. For example, discovery of the internal combustion engine which required gasoline as fuel turned men's attention to possible sources of oil. The applied scientist found ways of locating oil in the ground, while the petroleum technologist made the actual discoveries. In the hundred years since Edwin Drake drilled the world's first oil well near Titusville, Pennsylvania, in 1859, the technologists have taken this discovery very far. Oil is now a natural resource, a part of the available environment; but it can hardly be said to have been so a hundred years ago, although it was just as much in the ground then as now.

Another kind of technologist is the engineer; engineering is the most down-to-earth of all scientific work that can justify the name of science at all. In engineering the solutions of the technologists are applied to particular cases. The building of bridges, the medical treatment of patients, the designing of instruments, all improvements in model constructions of already existing tools—these are the work of the engineer. But the theories upon which such work rests, such as studies in the flow or "creep" of metals, the physics of lubrication, the characteristics of surface tensions of liquids—these belong to the applied scientist.

The industrial scientific laboratory is devoted to the range of applied science, from "fundamental research," by which is meant long range work designed to produce or improve practical technology, to immediate technological gains from which manufacturing returns are expected, for instance, the testing of materials and of manufactured products. Technological laboratories have been established in the most important of the giant industrial companies, such as DuPont, General Electric, Eastman Kodak, and Bell Telephone. The work of such laboratories is cumulative and convergent; applied science in such an institution is directed toward eventual technological improvement, the range of applied science and technology being employed as a series of connecting links to tie up pure science with manufacturing. University and foundation laboratories often serve the same purpose, but with the emphasis shifted toward the theoretical end of the scientific spectrum.

The development of technology has a strong bearing on its situation today and may be traced briefly. In the Middle Ages, there was natural philosophy and craftsmanship. Such science as existed was in the hands of the natural philosophers, and such technology as existed

was in the hands of the craftsmen. There was precious little of either, for the exploration of the natural world was conducted by speculative philosophers, while the practical tasks were carried out by handicrafts employing comparatively simple tools, although there were exceptions: the windmill, for instance. There was little commerce between them, however, for their aims were quite different, and the effort to understand the existence of God took precedence over lesser pursuits.

Gradually, however, natural philosophy was replaced by experimental science, and handicraft by the power tool. The separation continued to be maintained, and for the same reasons; and this situation did not change until the end of the eighteenth century. At that time, the foundations of technology shifted from craft to science. Technology and applied science ran together into the same powerful channels at the same time that the applications of pure science became more abundant. A craft is learned by the apprentice method; a science must be learned from the study of principles as well as from the practices of the laboratory, and while the practice may come from applied science the principles are those of pure science.

There is now only the smallest distinction between applied science, the application of the principles of pure science, and technology. The methods peculiar to technology: trial-and-error, invention aided by intuition, have merged with those of applied science: adopting the findings of pure science to the purposes of obtaining desirable practical consequences. Special training is required, as well as some understanding of applied and even of pure science. In general, industries are based on manufacturing processes which merely reproduce on a large scale effects first learned and practiced in a scientific laboratory. The manufacture of gasoline, penicillin, electricity, oxygen were never developed from technological procedures, but depended upon work first done by pure scientists. Science played a predominant role in such physical industries as steel, aluminum, and petroleum; in such chemical industries as pharmaceuticals and potash; in such biological industries as medicine and husbandry.

A concomitant development, in which the triumph of pure science over technology shows clear, is in the design and manufacture of instruments. The goniometer, for the determination of the refractive index of fluids (used in the chemical industries); the sugar refractometer, for the reading of the percentages by weight of sugar (used in sugar manufacture); the pyrometer, for the measurement of high temperatures (used in the making of electric light bulbs and of gold and silver utensils); the polarimeter, for ascertaining the amount of sugar in urine (used by the medical profession); these and many others,

such as for instance the focometer for studies in the length of objectives, the anomaloscope for color blindness, and the spectroscope for the measurement of wave lengths, are precision instruments embodying principles not available to the technologist working unaided by a knowledge of pure science.

4. *From Practice to Theory*

In the course of pursuing practical ends abstract principles of science hitherto unsuspected are often discovered. The mathematical theory of probability was developed because some professional gamblers wished to know the odds in games of chance. Electromagnetics stimulated the development of differential equations, and hydrodynamics function theory. Carnot founded the pure science of thermodynamics as a result of the effort to improve the efficiency of steam and other heat engines. Aerodynamics and atomic physics were certainly advanced more swiftly because of the requirements of war. Air pollution, which accompanies big city "smog," has led a number of physical chemists to investigate the properties of extreme dilution. Hence it it not surprising that many advances in pure science have been made in industrial laboratories: from the Bell Telephone laboratories alone have come the discoveries by Davisson and Germer of the diffraction of electrons, by Jansky of radio astronomy, and by Shannon of information theory.

Technology has long been an aid and has furnished an impetus to experimental science. The development of the delicate mechanisms requisite for the carrying out of certain experiments calls on all the professional abilities of the instrument maker. Such a relation is not a new one; it has long existed. The skill of the Venetian glass-blowers made possible many of Torricelli's experiments on gases. Indeed, glass can be followed through a single chain of development for several centuries, from the early microscopes to interferometers. The study of electromagnetics was responsible for the later commerce in electric power and the vast industries founded on it. But, contrariwise, thermodynamics grew up as a result of the problems arising from the use of steam in industry. We cannot afford to neglect in our considerations the economic support as well as the social justification which industry has furnished, and continues to be prepared to furnish, to research. The extraordinary rise of pure chemistry in Germany was not unrelated to the industry constructed on the basis of the aniline dyes, as well as cosmetics and explosives, during the nineteenth century.

The harm to practice of neglecting the development of pure theoretic

science will not be felt until the limits of installing industries by means of applied science and technology, and of spreading its results, have been reached. Science can to some extent continue to progress on its own momentum together with such aid as the accidental or adventitious discoveries of pure science in technological laboratories can furnish it. But there are limits to this sort of progress. Thus far the communist countries of the east have taken every advantage of the scientific developments achieved in the capitalist countries of the west. But after all, the applied science which the west has been able to furnish has been the result of its own preoccupations with pure science and with theoretic considerations which lay outside the purview of any practice. Industrial laboratories may occasionally contribute to pure science, but that is not their chief aim; and it is apt to be forgotten that such industries would not exist were it not for the fact that some centuries ago a handful of scientists with no thought of personal gain or even of social benefit tried to satisfy their curiosity about the nature of things. The restless spirit of science, never content with findings, hardly concerned with the applications of findings, is always actively engaged in pursuing methods in terms of assumptions, and must have some corner of the culture in which it can hope to be protected in its isolation. A wise culture will always provide it elbow room, with the understanding that in the future some amortized inquiry is bound to pay dividends. The ivory tower can be, and sometimes is, the most productive building in the market place.

Of course, applied science and technology cannot be independent of pure science, nor can pure science be independent of applied science and technology. The two developments work together and are interwoven. Gilbert discovered that the freely suspended magnetic needle (i. e., the compass) could be a practical aid to navigation at the same time he proposed that perhaps the earth was a gigantic magnet.

Problems which arise in the midst of practical tasks often suggest lines of theoretical inquiry. But there is more. Pragmatic evidence has always been held by logicians to have little standing. A scientific hypothesis needs more support than can be obtained from the practical fact that "it works." For who knows how long it will work or how well? What works best today may not work best tomorrow. A kind of practice which supports one theory may be supplanted by a more efficient kind of practice which supports quite a different theory. Relativity mechanics gives more accurate measurements than Newtonian mechanics. That use does not determine theory can be easily shown. Despite the theoretical success of the Copernican

theory as refined and advanced by Kepler, Galileo, and Newton, we have never ceased to use the Ptolemaic conception in guiding our ships or in regulating our clocks. However, if the practical success achieved by the application of certain theories in pure science cannot be construed as a proof of their truth, neither can it be evidence of the contradictory: workability is no evidence of falsity, either. Newton is still correct within limits. Practicality suggests truth and supports the evidence in its favor even if offering no final proof. The practical uses of atomic energy do not prove that matter is transformable into energy, but they offer powerful support. Hence, the use of a scientific law in the control of nature constitutes the check of prediction and control.

5. *Cross-field Applications*

We have treated all too briefly the relations of theory to practice and of practice to theory in a given science. We have also mentioned the productive nature of cross-field research. It remains now to discuss a last dimension of relations between science and practice, and this is what we may call cross-field applications: the employment of the practical effects of one science in those of another.

The applications of science have been greatly aided by the cross-fertilization of techniques. Radio astronomy, which has proved so useful in basic research, being already responsible for the discovery of "radio stars," and for adding to our knowledge of meteor streams and the solar corona, owes its inception to a borrowed instrument. Cross-field application has a long history, dating at least to the early half of the eighteenth century when distilleries in England brought together the results of techniques of producing gin acquired from both chemistry and theories of heat energy. Perhaps the most prominent instance of this is the way in which medicine has drawn upon the physical technologies. The use of the vacuum tube amplifier and the cathode ray oscillograph in determining the electrical potential accompanying events in the nervous system, the entire areas covered by encephalography and by roentgenology show the enormous benefits which have accrued to medical studies and procedures. Scintillation counters, developed and used in physical research, have been employed to measure the rate at which the thyroid gland in a given individual removes iodine from the blood stream; to measure the natural radioactivity of the body; to determine the extent of ingested radioactive compounds in the body; to assay the radioactive iron in blood samples. Chemistry has been an equally potent aid to pharma-

cology, which would have hardly existed in any important sense without it.

Other instances abound, and indeed multiply every day. In 1948 the Armour Research Foundation sponsored a Crystallographic Center, of interest to pharmaceutical corporations because of the crystalline nature of some of the vitamins. The invention of automatic sequence controlled calculators and other types of computing machines has seen their immediate application to atomic research and to military problems of a technical nature. Medical knowledge is being placed at the service of airplane designers, who must estimate just what strains their aircraft will demand of pilots. Perhaps the most graphic illustration of cross-field application is in scientific agriculture. Here hardly a single science can be omitted: physics, chemistry, biology—all contribute enormously to the joint knowledge which it is necessary to have if soils are to respond to management.

The cross-field applications of science usually work upward in fields corresponding to the integrative levels of the sciences. Applications found in physics will be employed in chemistry or biology, those found in chemistry will be employed in biology or psychology, those found in biology will be employed in psychology or sociology, and so on. The use of physics in biology has in fact brought into existence the science of biophysics, in which are studied the physics of biological systems, the biological effects of physical agents, and the application of physical methods to biological problems.

The cross-fertilization of applied science, the use of techniques, skills, devices, acquired in one science to achieve gains in another, has effects which tend to go beyond either. They add up to a considerable acceleration in the speed with which the applied sciences affect the culture as a whole. In the brief space of some several hundred years, western culture has been altered out of all recognition by the employment of applied science and technology to purposes of industry, health, government, and war. Much of the alteration has been accomplished by means of the cooperation between the sciences. We now know that the shortest route to an effective practice lies indirectly through the understanding of nature. If there exists a human purpose of a practical kind, then the quickest as well as the most efficient method of achieving it is to apply the relevant natural laws of science to it.

Engineering and Science: A Historical Review and Appraisal

JAMES KIP FINCH*

SOME THIRTY YEARS AGO the late Professor A. North Whitehead of Cambridge and Harvard, eminent mathematician, philosopher, and analyst of the forces which shape human progress, wrote: "The history of mankind is yet to be set in its proper relation to the gathering momentum of technological advances. Within the last hundred years, a developed science has wedded itself to a developed technology and a new epoch has opened."[1] Not only has the momentum of technological advances mounted in the years since these lines were written but it has become increasingly clear that their impact on modern life is of outstanding concern to men and nations.

Today the world faces what may be characterized as an era of technological nationalism; that is, one in which all nations, great and small, seek to achieve through the development of their resources the greater material welfare and higher standards of living that science and technology have brought to the Western World. Thus the problem of achieving both the most effective liaison possible between science and technology, especially between science and engineering, and an economic and social environment favorable to their continued development, has become of far more than mere academic interest and importance.

The pathways of the present, it is said, lead out of the past, and a study of the factors and conditions that led to the present scientific-technological liaison might reveal elements in this relationship which should aid in clarifying this modern problem.

As Whitehead noted, not only has the wedding of these two activities been recent but it was a *developed* science and a *developed* technology that were finally joined. For some forty-odd of the fifty centuries of recorded history what was long known as "natural philosophy" and the ancient practical arts we now know as engineering followed their

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own, practically independent, paths and interests. As Wolf puts it: "The invention of things and processes, on the one hand, and the discovery of their nature and laws, on the other hand, are activities which may be pursued more or less independently, and have been so pursued in the earlier history of civilization." [2] Indeed man's major material accomplishments from arch and truss bridges to steam engines, steamboats, and locomotives, from water wheels and the canal lock to the motor car and airplane, as well as many chemical and metallurgical processes and techniques, have been achieved through practical experience, through trial and observation, rather than stemming from any a priori scientific discoveries. A lack of full and exact understanding has never sufficed to inhibit man's creative instincts in meeting the challenging needs and wants of civilized life. Only in modern times and primarily in one field, electrical engineering, have discoveries in pure science been basic to the rise of a new branch of engineering endeavor.

Wolf argues that it has been "with the growth of knowledge that science and technology tend to become closely interlinked." The question thus arises: What forces and factors so long retarded this growth of a mutual interest in knowledge to which he attributes this interlinking? The answer seems to be quite clear. On the one hand, natural science as we know it today did not begin to develop until quite modern times, the sixteenth century. Thus for many earlier ages the limited and often mistaken speculations of earlier science offered little of any practical, useful value to the engineer or technician. During these same centuries, engineering also was largely limited in its applications to work in which there was little need for or urge to more accurate, scientific understanding.

Ancient Science and Engineering

What we today know as engineering had its origin in the work of the master builder, "the chief of works," who appeared in the valley of the Nile and in Mesopotamia with the dawn of civilization, at least by 3,000 B. C. Ancient Egypt has long been known as the Mother of the Practical Arts, for its early workers devised many of the hand tools we still use, mined copper on the Sinai peninsula, and pioneered in the production of such basic chemical products as pigments and dyes, glass and cements. But the Egyptian, having abundant labor and facing few challenges to progress, was intensely practical in interests, seeking only "how" best to use his resources and never asking the "why" of the simple rules and methods he derived from experience.

It was the inquiring Greek mind that, after the passage of a period of time longer than the Christian Era, at last sought to organize and systematize, to clarify and extend, these earlier empirical understandings. Again to quote Wolf: "Though scientific progress has sometimes prompted practical applications, yet more often pre-existing technical methods have supplied data for scientific discoveries." The Greeks created a science of geo-metry (i. e., land measurement) based on the practical rules evolved by Egyptian and other early workers. Similarly, a science of mechanics (i. e., contrivances) sought to explain some of many long known and used mechanical devices.

Only to a limited degree, however—and notably in surveying operations—did these Greek scientific beginnings find practical opplication and use in Greek life. Durant attributes this failure of Greek thought to stimulate the engineering and industry of the day to the lack of incentive in a slave civilization, to "the Greek disdain of manual work which kept everybody but the listless slave from direct acquaintance with the processes of production, from that stimulating contact with machinery which reveals defects and prefigures possibilities .... muscle was still less costly than machines."[3] Archimedes, whose popular reputation rested largely on his mechanical inventions, professed to regard them as unimportant and unworthy of record. Nevertheless, what Macaulay characterized as the "unprofitable" character of Greek thought failed to retard technical progress.

The use of slave power undoubtedly discouraged machine development through all of ancient times, but the Greek *architekton*, who planned and directed such works as water supplies and public buildings, harbor and city planning, and was both engineer and architect in the modern use of these titles, clearly possessed a clear conception of the technical and economic practicabilities that must guide the master builder's creative genius. Through experience and shrewd observations he arrived at some surprising, if purely qualitative, technical and economic understandings.

Contrary to popular belief the major role of science in engineering has been, as in the case of medicine, its contributions to the development within these professions themselves of more fully rationalized, exact, and quantitative techniques. In engineering the most obvious advantages stemming from this development is an avoidance of "overdesign"; each part or element of a machine or structure can be proportioned to meet the load coming on it, and a saving in material results. Every practical art contains the germ of such a practical science, but little need or incentive for its development in engineering was to evolve until well into modern times. In earlier ages only such

common and usually plentiful materials as timber, stone, and brick were available to the master builder. Metals were limited and too costly except for tools, weapons, and fastenings. The structures of the day involved only the use of such long known and familiar elements as the post and lintel, i. e., columns and beams. It was centuries before new structural forms and the advent of fuel-consuming steam engines, which supplemented early man power and the uncertain gifts of wind and water, provided an urge to more exact understanding.

To be sure, the Romans, long hailed as the greatest of ancient engineers, were the first to make extensive use of the arch which had been known for centuries. But the light loads that prevailed until the advent of railroads raised no critical design problem. The solid, spandrel filled arch bridge did pose major problems of flood-clearance and especially in securing safe foundations; yet few such arches have ever failed because of deficiencies in superstructure, although their analysis involves many uncertain factors and, even today, is still approximate. As Séjourné, the great modern master of the stone arch puts it: "*On fait une voûte d'après les voûtes faites; c'est affaire d'experience.*" ("One plans an arch after arches that have been built; it is a matter of experience.")[4] Thus while Roman roads consolidated the Italian peninsula and were repeated in the provinces and while the remains of over 200 aqueducts are scattered over the Roman world, the Roman remained distinctly a practical builder and organizer. Unlike the Greek, he had little interest in science, yet he made engineering a vital instrument in building Roman life and spreading Roman civilization throughout the world of the day.

The Middle Ages and Renaissance

Similarly, the Middle Ages, which may be characterized as an Age of Craftsmanship, again emphasize the fact that experience has long been the great teacher. Modern science was still to be born when later medieval times witnessed some of the most outstanding works and technical developments in man's long history. For example, the "skeleton-stone" Gothic cathedral, evolved solely through trial and observation, still remains man's greatest triumph in stone construction. The *ingeniator*, ingenious builder of the massive medieval fortress-homes, later provided the title for the engineer when engineering finally split off from what we know today as architecture. Gunpowder, whose discovery may, perhaps, be attributed to the pseudo-scientific activities of the alchemist, brought about a new era of military engineering which stimulated metallurgical progress in the casting of

cannon, advances in surveying, and the development of new means of defense. The birth of a machine era and the widespread use of water and wind mills that came in late medieval times marked the initiation of what was to become a new, vitally important, and ultimately revolutionary change in man's way of life.

When the stream of Western life moved on to the so-called Italian Renaissance of the fifteenth and sixteenth centuries these new interests came in conflict with "humanism," a turning back to the culture of Greece and Rome for inspiration and guidance. Architecture began to assume the attributes of a fine art, and the artist-architect-engineer of the Renaissance adopted the work of an earlier Roman writer, Vitruvius (c. 15 B. C.), as the architectural bible of this classical rebirth. This artistic-eclectic approach was to characterize architecture until the last twenty or thirty years. To a major degree it also carried with it, for several later centuries, some of what we now know as civil engineering activities. Thus a major bridge innovation of late medieval times, the segmental arch, was still-born, and "appearance design" prevailed in the scant attention given such works by Renaissance writers and some later authors.

This movement, however, soon lost ground in the military field. Gunpowder tumbled down the medieval fortress and replaced it with earthwork fortifications having little aesthetic appeal. Techniques unrelated to an artistic interest became important. Specialization was inevitable. Military engineering may be said to have largely reached independence with de Marchi's text of the mid-sixteenth century which, though titled *Military Architecture*, was the work of a professional soldier who lacked either artistic or classical ties. Similarly, the rise of canal building and the evolution of what is probably the greatest of hydraulic inventions, the canal lock, in the fourteenth century, introduced another element foreign to the artistic-humanistic approach.

In late medieval times the development of machines as a means to increased production stirred other new and specialized interests. This movement was apparently prompted by a decline in slavery and a revival of trade between the awakening West and the Near East. The reopening of the Mediterranean to western commerce, via Italian intermediaries, and an urge to increase exports in exchange for eastern products led to the birth of a new mechanical era. Water mills appeared wherever water was available. Pumps for drainage or to supply the water needs of growing towns and cities; imports of earlier Far Eastern innovations, from wheelbarrows and horse collars to wind mills and printing; plus new inventions—these mark the birth of a new

industrial era. Early manuscript records are filled with mechanical speculations. Many of the first printed engineering books are so-called "Theaters of Machinery." Yet all these remarkable medieval technological advances were achieved with little or no interlinking of theory and practice.

The Renaissance, however, also witnessed a new birth in natural science. Science, it has been said, was at last made free; the restrictions imposed by the Roman Church failed to restrain man's scientific curiosity. Although the limitations of Scholasticism and the illusions of alchemy and magic persisted for many years, the sixteenth century marked the beginning of a new understanding of nature. This understanding grew and ultimately aided the engineer in replacing his earlier, purely qualitative, empirical, experienced-derived design techniques with the more exact, largely quantitative procedures of today. Yet the entire seventeenth and eighteenth and the first half of the nineteenth centuries were required to bring about the wedding of developed interests that Whitehead recalled. The courtship of science and engineering occupied at least two centuries.

The Rise of Natural and Engineering Science

It appears, as Wolf notes, that the birth of modern science was not inspired solely by "scientific curiosity." Scientific knowledge was not valued by Bacon (1561-1626) "for its own sake," Wolf remarks, "but as a potent instrument for improving the lot of mankind by means of inventions which might result from it." The various academies of science—the Italian Academy of Experiments (1657), the British Royal Society (1662), the French Academy of Sciences (1666)—which made possible an exchange of ideas in a period when the universities were still given over to Scholasticism, unquestionably were granted state support on similar, utilitarian grounds.

Actually, however, the pioneers of science soon became absorbed in the mathematics of astronomy and physics; not only was little attention given to practical applications, but the descriptive sciences, not being susceptible to a mathematical approach, were largely ignored. Thus is was an engineering author, Agricola (1494-1555), a contemporary of Copernicus (1473-1543), who not only produced a mining-metallurgical classic, *De re metallica* (1556), but pioneered in geological and minerological studies. Later Gautier (1160-1737), who wrote the first book devoted solely to the building of bridges (1716), was to complain bitterly not only of the shortcomings of the architectural approach in design but also of the highly mathematical

preoccupations and lack of interest in practical problems of *les scavans*.[5] Belidor (1697-1761), who appears to have been the first to use the term "engineering science" in his *La Science des Ingénieurs* (1729), although more of a pacemaker, likewise deplored this lack of cooperation.[6] Even as late as 1826 Navier (1785-1836) was to remark in his *Leçons*, a structural engineering classic, that the theoretical mechanicians "have hitherto been more useful for the progress of mathematics than for the improvement of the art of construction."[7]

It seems clear that the early, notably French, workers in engineering science were neither inspired by scientific curiosity nor dependent on scientific discoveries. In the construction field the same limitations of materials and structural forms of earlier times still prevailed. The major problems of the day were constructional and primarily due to a lack of power equipment. For the rise of useful techniques of design, theory had to be supplemented with data based on actual tests of materials and devices. Furthermore many earlier theories were in error. What are now regarded as the most elementary, almost obvious rules of statistics involved not only many men, both scientists and engineers, but centuries of time for clarification. Thus the first correct design of a simple beam to carry a given load was not achieved through a correct analysis of this simple problem of coplanar statics. Belidor, in spite of erroneous mechanical assumptions, arrived at a correct result by a proportional comparison using tests of a similarly loaded beam. It was not until almost a half-century later that another military engineer, Coulomb (1736-1806), whose contributions are also recalled in the electrical unit named in his honor, first stated correctly "the common theory of flexure."

The engineer's development of testing techniques, machines and instruments of measurement, was thus essential to the rise of a practical structural mechanics, but it also undoubtedly had some influence on the increasing resort to instrumentation and actual measurement which characterized scientific progress in the eighteenth century. Natural science and engineering science were unfolding simultaneously, each making its contributions to the clarification of common interests. In this advance actual testing and measurement played a vital role both in revealing new and correcting earlier erroneous ideas. Thus not only in simple static problems but in such practical dynamics as the flow of water in open channels from rivers and streams to aqueducts and canals, trial and experience over many years were required to correct earlier misconceptions.

The modern reader is apt to dismiss as futile and incompetent Belidor's efforts to analyze the performance of water wheels and

water wheel pumps in his later four monumental volumes on *Architecture hydraulique*.[8] Yet, searching for what he refers to as "the perfection" of these devices and thereby pioneering in the development of the mechanics of machines, he was also misled by the erroneous scientific ideas then prevalent. These misconceptions were slowly corrected; but it was the advent of the steam engine using costly fuel, the production of iron in quantity and at a cost that made its wide use possible, followed by the birth of the iron bridge (1779) and the later truss forms, that added far greater incentives to the continued correction and extension of engineering science. This development, it might be noted, foreign to architectural interests, finally led to the complete divorce of the ancient identification of architecture and engineering.

European vs. American Interests

The Revolution in France emphasized science, and, in general, later French contributions to engineering design took on a highly mathematical and theoretical character little related to the technical problems of the day. British engineers, however, taking over engineering leadership as the nineteenth century advanced, made notable progress in the development of testing and practical mechanics. The earlier contributions of Tredgold, Moseley, Fairbairn, Hodgkinson, and others were brought together and extended by Rankine (1820-1872), the famous Scotch professor of civil engineering at Glasgow, in his *Applied Mechanics* (1858). Watt's efforts to save fuel through his invention of the separate condenser for mine pumps and his later development of the rotative engine led to a new interest in heat, and a modern theory of heat followed. Rankine, in his *Manual of the Steam Engine* (1859) endeavored to give a practical turn to these new ideas but Rankine's texts also tended to be highly mathematical and left much to be done in bridging the gap between theory and practice. In fact, in sharp contrast to American interests and trends, European engineering has continued to emphasize what American engineers regard as a highly mathematical, theoretical interest in the techniques of design. This has been primarily due to a difference in what may be called the "labor-material balance."

In general, in Europe skilled labor has been plentiful but materials relatively expensive. Contrariwise, in the United States skilled labor has always been scarce and costly while materials have been plentiful and relatively low in cost. More exact techniques of design lead to refinements which are usually more laborious to produce, although they result, as we have suggested, in a more economical use of materials.

European workers, therefore, have led in developing more advanced techniques of design, while ease and rapidity of construction, the labor-saving machine, mass production, and industrial "know-how" have characterized the remarkable rise of American engineering and engineered industry. From the famous American pin-connected truss bridge, which could be erected in a few days, to the equally famous "American system" of manufacture, the economic urge to save labor and to minimize the period of unproductive investment by speeding up operations has dominated in engineering and industry.

Natural science thus long played a distinctly minor role in the American surge to technological and industrial leadership. In fact, the United States cannot claim leaders of science in the nineteenth century comparable in stature to those of Europe. The United States has excelled in the application of new ideas in production and manufacture but, with few exceptions, not only have scientific advances originated abroad but both newer design techniques as well as many new devices and products—textile machines, dynamos, steam turbines, internal combustion engines, and motor cars, steel, concrete, and many modern chemical products—have been of foreign origin. Although frequently known as "scientific," the American engineering schools, the majority established shortly after the Civil War, were, by contrast with European practice, limited in their teaching to the simplest mathematical and scientific ideas. American engineers commonly regarded European engineers as not having "a practical hair in their heads"; "pure" science received but lip-service in the United States and was left to the dedicated but usually ill-paid individual worker.

In sharp contrast, fortunes were being made in steel and oil, in railroads, and in electrical developments (the telegraph, telephone, and lighting), which some scientists regarded as mere applications of the hard-won discoveries of self-sacrificing scientific workers. The fact that some electrical inventions were based on phenomena first disclosed through earlier scientific discoveries led several eminent scientists to unwarranted conclusions, historical misrepresentations, and intemperate statements. For example, H. A. Rowland (1848-1901), Professor of Physics at Johns Hopkins, wrote: "It is not an uncommon thing especially in American newspapers, to have the applications of science confounded with pure science; and some obscure American who steals the ideas of some great mind of the past, and enriches himself by the application of the same to domestic uses, is often lauded above the originator of the ideas, who might have worked out hundreds of such applications, had his mind possessed the necessary element of vulgarity." Fifty years later Henri Poincaré in *Science et methode* (1912) repeated

the same indictment: "One has only to open one's eyes to see that the triumphs of industry, which have enriched so many practical men, would never have seen the light if only these practical men had existed, and if they had not been preceded by disinterested fools who died poor." To which he adds a statement by Mach: "What these fools did was to save their successors the trouble of thinking." One wonders, on the one hand, what particular thefts these critics would cite if called upon to document their observations and, on the other, at the remarkable contributions which the alleged thieves have made to modern life without "the trouble of thinking."

Science and Progress

Perhaps the best example of the fact that advances in pure science alone do not insure continued industrial and economic progress and higher standards of living is reflected in the British experience of the Victorian and pre-World War II period. Certainly no nation ever numbered a more distinguished group of scientists than those which Britain produced in these years—from Davy, Faraday, and Darwin to Maxwell, Kelvin, and a host of other outstanding men. Yet the industrial and economic leadership of Britain, the motherland of the Industrial Revolution, deteriorated during this same period. Undoubtedly a number of factors contributed to this technological and industrial decline. As agriculture gave way to industry, manufactures provided the goods in exchange for raw materials and food products in a foreign trade carried on in her own ships and in which Britain long met little or no competition. She thus continued to exploit the same industries—textiles, iron and steel and their products—on which the Industrial Revolution had been based, and found little incentive to a more diversified output. Furthermore, colonial opportunities offered a more lucrative return for the investment of capital gains than was apparent in possible technological improvements in her home industries.

While America was moving ahead through industrial "know-how" and a wider sharing of increased productivity, there was little modernization of British industry and living standards remained largely static. As late as 1947, Sir Norman Angell remarked that the British workman was convinced that "for a man to produce his maximum is to do some mate out of a job and that labor-saving machinery may make a profit for the capitalist but inevitably increases unemployment." [9] To a major degree, British scientists also retarded technological progress by what can only be characterized as a "high-hat" attitude—the

notion that industrial and economic applications of science automatically follow any scientific discovery, so that applications can be left to technicians and other "navvy" (i. e., laborers') occupations. Only since World War II has the situation changed and Britain, adopting modern technical and manufacturing advances, again joined the ranks of the progressive industrialized nations.

World War I, however, had resulted in a remarkable change in the relationship of science to engineering and industry in the United States. It was not, as Haynes remarks, until after the turn of the century and the coming of World War I "that the American people first began to identify the contributions of science with the progress of our material civilization."[10] While invention had long been regarded as the key to material progress, about this time scientific research began to be thought of as the key to invention. Two developments furthered this identification, namely, the rise of organized, industrial-scientific research in the chemical and electrical industries, and the need for more extensive and exact design procedures in the older branches of engineering.

Earlier inventions had come about, it has been said, through the marriage of an inventive idea with the means for its realization; the idea came first, a search for means followed. The rapid growth of the American chemical industry in making good the World War I shortages of many essential chemicals previously imported from Germany gave emphasis to a reverse relationship. It became clear that these German products were largely the result of organized, industrial-scientific research and that such research could uncover data for which practical applications and use might be found. Furthermore, through organized research an industry could, perhaps, anticipate innovations by competitors or by individual workers who lacked these organized resources.

Industrial research—it has been called "the invention of invention"—started in the United States shortly before World War I. As early as 1902 the DuPont Co. established its first research laboratory; Whitney became director of the General Electric Laboratory in 1904; and in 1907 the American Telegraph and Telephone Co. started a laboratory in Boston which was later moved to New York and, in 1925, became the famous Bell Laboratories. In 1912 the Eastman Kodak Co. called C. E. K. Mees from England to become director of their research interests. The writer can recall an early address by Dr. Mees in which he outlined his ideas, then regarded as revolutionary and radical, on organized research, on planned team work, on periodical conferences for the exchange of ideas, and on the use of technical

assistants to carry out routine tests and measurements. One is reminded of Taylor's work in industry, the so-called "scientific management" of the 1880's and '90's, and of the advances in "organized construction" which were likewise under way at the turn of the century.

The major interests in these laboratories was directed, as it still must be, to production, to cost-quality problems. In describing the development of the telephone, W. H. Martin of the Bell Laboratories remarks that invention is first followed by the production of a practical, useful, manufacturable, and marketable product. Studies looking to the possible advantages of changes and modifications, to improvements in quality and savings in cost, follow. Means of measurement, permitting the more accurate evaluation of the factors disclosed by pure try-it-and-see techniques, are devised and lead to further refinements, to an improved product produced at a lower cost.[11] These are production problems best solved by team work, but the large laboratories also increasingly sponsored fundamental research that might lead to new products. In 1928 the DuPont Co. established a laboratory devoted to more basic studies and, in 1942, fourteen years later, reported that half of their gross sales resulted from products either unknown or not commercially manufactured in 1928.

While these research interests have been especially characteristic of the chemical and electrical industries, which offer exceptional opportunities to develop and market new products, the older branches of engineering have progressed largely through continued inventive-evolution rather than new and spectacular innovations. The need for organized research has been less evident in these older branches, and, patents are few and far between; nevertheless, constant improvements in techniques and equipment have made possible bigger and better works as well as more effective provision for long established and essential needs and services. Mining engineers have been challenged by problems of mechanization in handling deeper hidden and lower grade ores. The growth of the petroleum industry, in particular, has led to more scientific techniques of exploration. In the civil and mechanical fields, new needs have accelerated the search for further refinements and the extension of earlier design techniques, that is, to advances in engineering science. The advent of an Age of Concrete and Reinforced Concrete with increasing attention to "continuous" forms in bridges and other structures, the invention of the airplane with emphasis on weight-saving and with new structural and mechanical problems forcing increased attention to more advanced techniques of analysis and design, and the replacement of piston engines by the turbine—all these have been factors in this movement. Similarly, the

increasing scale of engineering works—great bridges in which hitherto relatively unimportant "secondary" and other forces could no longer be ignored or met by generous "over-design," great steam plants using a ton of coal a minute in which reducing fuel by a mere fraction of one per cent saves thousands of dollars a year—and other, basically economic factors gave purpose to a search for more exact design procedures which has since gone forward at ever increasing pace. The American engineering graduate of the turn of the century finds that his early training has left him ill-prepared even to follow the design techniques of today.

Conclusion

In the years that have passed since the birth of organized industrial research and the advent of these new techniques, the proponents of pure science, seeking support for essential basic research, have not hesitated to further the idea that the accomplishments of these years show that technology is simply a child of science; hence, they claim, all that is needed to insure continued technological and industrial progress is more and more scientific research. After a long period of comparative neglect science has thus been exalted to a position of omnipotence in American life, with billions being poured out today by government and industry in support of scientific research.

On the one hand, there can be no question that the role of science in our material life has been extended far beyond that of simply aiding in the development and extension of more exact techniques of design. Scientific discoveries have increasingly made possible not only the improvement of creative ideas but also their realization, and in some cases have prompted their conception. There are, however, some thoughtful scientists who feel that science has become a mere appendage of technology and are disturbed by the widespread use of the term "science" or "scientific" for many interests, activities, and products which have little or no connection with truly scientific pursuits.

Although modern science, as we have noted, owed much of its early support to the hope that useful discoveries would be made, many men of science insist that science must be pursued for science's sake alone. Aristotle is quoted as saying that true science is the search of nature in the spirit of pure scientific curiosity. "The true man of science has no such expression in his vocabulary as useful science," said Poincaré, "if there can be no science for science's sake there can be no science." That such a dedicated search for further understanding

of nature and nature's laws is essential to man's quest for a good life through increasing mastery of his environment cannot be denied. Yet it is clear that, basic and essential as is pure science, research in science alone provides no assurance of continued economic and social progress.

Progress in man's search for the good life does not rest alone on knowledge but upon the use we make of knowledge. Knowledge becomes power only when it is utilized and applied. As an eminent British industrialist, Sir Ewart Smith, observes: "Any real basic knowledge which is envolved is, broadly and relatively quickly available to all, and it is therefore upon technological skill in application that the progress of industry and, consequently, the economic position of the nation will mainly depend." [12]

Scientific knowledge in particular has long been freely available to all. Recent efforts to secure secrecy, as in the case of the nuclear bomb, seem doomed to failure. Similarly, technical advances cannot be confined by national boundaries, as witness Britain's efforts to prevent the export of her earlier textile inventions. Indeed, far from jealously guarding its precious "know-how," the United States today endeavors to aid the less-developed nations of the world through exporting the technical methods and equipment which are basic to its own material prosperity and welfare. But it is becoming increasingly clear that factors other than science and purely technical advances are involved in the realization of higher standards of living.

The United States has achieved its present position of industrial power not through superior scientific or technical knowledge but through using such knowledge most effectively in utilizing resources and in meeting needs peculiar to its own economy, people, and environment. Other nations face a similar problem in seeking the best path to follow in the application of knowledge to their own special needs, aims, and conditions.

In short, as Sir Ewart Smith so clearly states, scientific and technical advances result in economic and material progress, in higher standards of living, only when skillfully coupled with knowledge of other, quite different but vital requirements basic to intelligent application. In sharp contrast to the scientific worker who concentrates his efforts on the study of a special segment of his field, the professional engineer must understand and give due consideration to a wide range of pertinent factors. These include not only the relative costs, qualities, and special advantages of various materials and a knowledge of available resources of labor and equipment, but a careful analysis and appraisal of present and possible future economic and social needs. The techniques of design are but the tools with which the professional skill of the

engineer shapes creative possibilities best to meet economic and social needs and practicalities.

Thus the mere export of American methods and machines, of the products of research and the techniques of design, to the less-developed nations of the world, will not solve a problem which is basically one of the intelligent adaptation and application of those advances to the special conditions of other peoples who possess different resources and face different economic and social problems. As Professor Whitehead stated: "The history of mankind is yet to be set in its proper relation to the gathering momentum of technological advances." Further scientific and technical research will continue to add to "the momentum of technological advances," but the outcome will depend primarily on how skillfully these advances are applied in meeting the varying needs and conditions of men and nations.

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Engineering and the Scientific Revolution

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HARDLY MORE THAN a couple of generations ago sound engineering practice consisted for the most part of a combination of manual dexterity with a rather large number of empirical rules approved by long, evolving experience. Yet engineering had already been invaded by formulae or principles which were not merely empirically derived: thermodynamical calculations, the procedures for determining structural stresses, part at least of the theory of fluid-flow as well as the whole new development of electrical engineering were products of theoretical investigation which had been confirmed by experiments and in due course by experience. In such cases as these we may properly speak of engineering's being dependent upon science. It is true that the practical engineer of the nineteenth century derived his figures rather from tables and graphs than by going back to pure theory; what matters is the origin of the rules he applied in a theoretical analysis rather than in a codification of successful empirical practice. And it is significant that all the time the trend was away from customary rules to theoretically-derived ones; by and large the engineer did not distrust science: he welcomed it.

To all this the sixteenth and seventeenth centuries offer no parallel. Theoretically-derived rules were employed in no branch of engineering, and there is little sign that practical men felt any need for them. Custom was powerful; next to that the most important consideration was not scientific but aesthetic. Alteration in taste is really the only fact that need be invoked to account for changes in building practice at this time—though the greater use of metals for taking tensile stresses, for example, ensured that Baroque architecture was not quite like that of the ancients. Perhaps the nearest parallel to the nineteenth century situation—and it is outside engineering—was the fact that gunners were offered (from 1638 onwards) tables of elevations and

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ranges calculated on the assumption that projectiles follow a parabolic trajectory. But the assumption was false, since projectiles are subject to air-resistance, and the tables were quite inapplicable under seventeenth-century conditions. Before scientific ballistics could be useful it was necessary to develop both a more complex mathematical theory and artillery of predictable, long-range performance; neither was achieved before the nineteenth century.[1] Tables of proportions were used in engineering, naturally; by shipwrights, possibly by bridge-builders, certainly by architects. But such tables were governed by rule-of-thumb (or rule of three), or by aesthetic canons of proportionality; they had no analytical justification.

For all its swift advance seventeenth century science still lacked the depth of precise, quantitative information that alone is useful to engineers. Its achievements were either conceptual or at sublime heights of mathematical theory. In his *Principia* (1687) Newton could equate exactly the centrifugal force of each planet at any point in its orbit with the sun's gravitational pull at that point; yet he erred by a factor of two in calculating the height to which a given head of water would spout.[2] The great discoveries of mathematical physicists were not merely over the heads of practical engineers and craftsmen; they were useless to them. Christian Huygens's theoretical enquiry into the isochronism of the pendulum had no effect at all on the actual improvement of clocks, although his mechanical concept (the regulation of a train of clockwork by a pendulum) was of the first importance and the starting-point for a succession of practical improvements by working clockmakers.[3] The well-known studies of the engagement of gear-teeth by the Danish astronomer Ole Römer and others in the French Academy of Sciences fall into a different category of practical usefulness, for millwrights (if they ever heard of them) were quite uninterested in constructing more perfectly shaped gears, and even if they had wished to do so they lacked machinery for cutting the required epicycloidal profiles.

In this instance the millwrights were much more concerned with substance than with form. Wood was an unsuitable material to use for gears; on the other hand, metals such as iron were impossibly difficult to work except on the small scale, and were almost equally subject to fracture. Such a limitation of materials and of means for making the best use of available materials was a principal problem confronting the seventeenth century engineer, as all his predecessors. It cut down the extent of his building-spans; it reduced the carrying-capacity of his ships and forced him to rebuild them after as little as ten years' service; it excluded the use of high temperatures and

pressures; it made accurate dimensions unattainable; it made his machinery either excessively cumbersome or weak and rickety. This limitation has been overcome subsequently chiefly through the use of concrete and metals, that is, by chemical knowledge. Now chemistry took swift strides through the seventeenth century both in theory and practice, but again not in such terms as to yield cheap steel or phosphor-bronze bearings. Even the problem of smelting a coarse iron with coal could not be solved—and it was a practical man, not a chemist, who took the first steps to making coal-smelting practicable, in 1709. Before the mid-nineteenth century there was no useful body of chemical theory from which useful consequences could be drawn to benefit metallurgy; hence the one great amelioration in material resources that came before then (the availability of cheap cast-iron) came as yet another triumph of craft empiricism. Sophisticated engineering was thus out of the question until materials superior to wood, brick, stone, and crude iron were at hand in quantity, so that the use of physical theories in engineering was in any case delayed by the rather slower progress of chemical theory.

At the same time the engineer himself was notably blind to the importance of mathematics, which was the foundation of the seventeenth century success in physics. The association of the engineer with the slide rule is quite recent. True, there was little enough for the practical man to compute: but what he did he did badly, clinging to antiquated rules-of-thumb, as Pepys found when he looked into the ways used to compute footage of timber for the Admiralty. No one ever knew how many gallons a cask would hold, or what the displacement of a ship would be. Until science took a hand, levelling and survey were carried out extremely crudely, and the reckoning of quantities on any job was full of arithmetical errors. Until after about 1650 the engineer, like every other technologist, was only rarely educated or mathematically literate. So long as multiplication and division remained almost intolerable burdens it was hopeless for engineers to attempt to borrow from science. For the ability to reason "If one windlass will raise 100 lbs., I shall need ten to raise half a ton," or some acquaintance with the laws of pulley and lever can hardly be reckoned as indebtedness to science at this stage of the game. The engineer, indeed, was hardly yet equipped to formulate his difficulties and his hopes for progress in a way that a mathematician or physicist could comprehend.

In consequence, the initiative towards mathematical accuracy, and often towards other improvements too, had to come from above. It was the philosopher Galileo who conceived the possibility of a mathe-

matical theory of structures, showing in his *Mathematical Discourses* (1638) how strength could be calculated in proportion to size, not merely guessed at.[4] It was the statistician-economist William Petty who endeavored to introduce a radically new principle of ship-building, the catamaran hull.[5] It was the physicist Huygens who saw the possibility of making heat do work, while numerous mathematicians from Galileo's pupil Benedetto Castelli onwards investigated fluid dynamics, whether in the more specialized field of river-control or in the more general applications tackled by Huygens and Newton. Indeed, scientists seem to have been far more awake to the broad prospects of applying science to technology than were the technical experts. It was unfortunate that their most ambitious attempts in this direction failed. Hydraulic engineering was not put on a scientific basis. Huygens abandoned his experiments with the gunpowder engine so that, although there is a direct connection between his device and Newcomen's, the successful fruition of the heat-engine owes virtually everything but the concept of atmospheric pressure to practical engineers.[6] Petty utterly failed to win over the shipwrights with his experimental boats—and in any case his new design was not born of scientific principles. As for Galileo's theory of structures, it involved a fundamental misconception and would have led to failures if it had even been applied.[7] And in spite of further advances in the same theory from the end of the seventeenth century and later, a long time elapsed before calculation impinged on bridge and building design. It is significant that it is hard to hit upon anything that renders the mathematician Wren or the experimenter Hooke more scientific in their approach to design than were other contemporary architects.

In brief, the naive notion of some philosophers that the mathematical and experimental methods of science would revolutionize civil engineering and the construction of machinery within a few decades was falsified. The new-made world of which they dreamed was not to be attained so easily. Late seventeenth century engineering was much like that of the time of Agricola and the first machine-books.[8] Some recent feats were indeed beyond comparison with anything in the immediate past, notably the Languedoc Canal; but they involved no more than slogging at pick-and-shovel, masonry, and carpentry. Perhaps the greatest civil engineering improvement of the seventeenth century was the introduction of the wheel-barrow, which the Chinese had used for centuries! Wind- and water-power were more extensively exploited than before but with machines of established design and construction. Ships about doubled in size, but shipyards did not alter. Towns were still watered by placing a wheel in the stream of a

river and connecting it to a set of backyard pumps, and so on. Indeed, it is difficult to imagine how, without new sources of energy or new materials, there could be any very rapid change in engineering—even if more complete and reliable scientific theorization had been accessible. As it was, technical invention could consist of little more than a re-shuffling of existing, familiar knowledge and practice; just as some engineering enterprises and some machines could become larger merely because greater wealth and continuity of purpose were at hand to push them through. Not that the age of early science was lacking in technical inventiveness—though often enough devoted to such chimaeras as perpetual motion and the flying-machine; it is found in the engineering of every craft, from improvements in the printing-press, through the stocking-frame and the clockmaker's gear-cutting engine to the use of shearing and rolling mills in the iron industry. The scope and resource of the machine-maker were continually increasing, preparing the way (one might say) for his next great crashing of barriers in the textile industry. But for the sixteenth and seventeenth centuries the pattern remained largely unmodified; there was no engineering revolution alongside the scientific revolution, barely indeed a minor disturbance.

Does this mean that engineering owed nothing to science beyond the concept of the pendulum clock and of the atmospheric/heat engine? Not quite, though some of the debt was potential rather than actual. In rather indirect ways the scientific notion of exactitude began to affect engineers. This is most evident in survey and levelling, where serious errors in measurement could have brought catastrophe to such schemes as the Languedoc Canal and the draining of the Fens.[9] Partly it was simply a question of training field-surveyors in basic geometrical procedures.[10] Not less important, though it came rather later, was the borrowing of the astronomer's tools, notably the telescopic sight with cross-hairs devised by Picard in 1668. To this must be added the bubble-level invented by the scholar Thevenot a few years earlier.[11] More years passed before the surveyor became much concerned to refine his reading of angles, but when he did he once more borrowed the astronomer's vernier and micrometer screw. In turn, through *his* dependance upon the skills of the instrument-maker and clockmaker, the astronomer impelled fresh exactitude into their miniature craft methods. Just as Boyle's airpump (1658),[12] constructed for him by Robert Hooke (who was later Curator of Experiments to the Royal Society and the progenitor of many other devices, including the universal joint), was the antecedent of all later machines depending on a piston accurately fitting in a true cylinder—and in turn derived

from the backyard pump—so the clockmaker's gear-cutter and the instruments-maker's dividing-engine [13] were the ancestors of many later machine tools. Long before large-scale engineering could attempt such tasks these craftsmen were making fairly accurate gears, turning exact steel shafts, cutting good screws, measuring to an accuracy of one part in a quarter of a million or better, and so on, for their scientific employers and even for the open market. From their ranks some great engineers like Smeaton and Watt were recruited, and the total effect of their craftsmanship on productive mechanical engineering may well have been considerable. For besides their craftsmanship the leading men were scientifically educated (in England a few of them attained to the Fellowship of the Royal Society) and well able to look beyond the bounds of manual dexterity; indeed, they were the first of all craftsmen to perceive the fundamental truth that a machine can be more precise than any pair of hands.[14]

However, the fruition of this trend only occurred towards the end of the eighteenth century when a new race of engineers was already active on the scene, on whom instrument-makers like Jesse Ramsden or watchmakers like Thomas Mudge probably had little direct influence. It may be more proper to enquire whether, from a much earlier time, science did not influence engineering in a more subtle, pervasive fashion. Since it is fairly clear that engineers did not readily adopt mathematical analysis from science, might they not at least have borrowed the experimental method, or inhaled a still more nebulous "scientific spirit"? The difficulty is to know how this could possibly be estimated. Building a medieval cathedral was in some sense an experiment; it might stay up or fall down. Craftsmen have always experimented and talked of experiments: but not scientifically. It would be impossible to prove that there were more engineering experiments between, say, 1500 and 1700 than between 1200 and 1400, and meaningless if it could be done because the two periods differ so greatly in other respects than their science. Suppose one could show that bridge-builders were more daring in the seventeenth century than the fourteenth: what would this prove with regard to science? Is there any meaning in saying that Palladio's trusses or de l'Orme's composite beams owe anything at all to science? And so forth: everything becomes intangible, so that one might as well try to prove the opposite, that an empirical, engineering spirit begins in the sixteenth century to pervade science (some people seem to want to say both at the same time!)

To be more specific, most people would agree that the notebooks of Leonardo da Vinci undoubtedly demonstrate a true scientific out-

look and the sound, original design ability of a true engineer. To some extent also it is obvious that these two interests or qualities are related in Leonardo; the Leonardo who was interested in dynamics (leaning very heavily on the medieval philosophers) overlapped with the Leonardo who was interested in ballistics, the construction of cannon, and fortification, but that his sketches and notes on machinery and other engineering topics were greatly marked by his concern for abstract scientific questions would be hard to demonstrate. Leonardo did what other inventors of new machines did; he re-assembled the familiar parts (tappets, screws, cams, gears, and so forth) to achieve new combinations of motions, and so new mechanical effects. As an engineering designer Leonardo was most nearly modern in his notes on stresses in structures, for he tried to formulate problems geometrically and so to solve them; but his lack of success meant that he could not have applied geometry to architecture, even had he had the opportunity. Leonardo, it is true, is an enigma; but what is obscure in him, who was engineer and scientist combined, and concerning whom ample evidence survives, must be far more opaque with respect to other engineers of whose scientific interest and attainments nothing is known, and whose personalities are almost wholly obscure.

Nebulous questions can only receive nebulous answers. We may be content, if we choose, with an intuition that practicing engineers and inventors absorbed something of the more scientific environment of the seventeenth century, even though this can hardly be proved; but we must at the same time allow for other economic and educational factors that pressed upon them, and not forget the effects of accumulating knowledge and experience in evaluating their attainments. But what of the other side of the question—what did engineering contribute to science? Again, the wider the terms in which such a question is posed the more vaporous and unanswerable it becomes. There is evidence that scientific propaganda in the seventeenth century stressed the potential benefits flowing to medicine and technology from scientific enquiry; there is far bulkier evidence that in their work scientists investigated only the questions that took their fancy, from mathematical physics to the physiology of reproduction; in other words, science was apt to be concerned at any moment not with urgent problems of engineering and agriculture but with matters that scientists considered significant and interesting. And there are distinct statements that the Fellows of the Royal Society and the scientific Academicians in Paris alike resisted attempts to tie them down to useful researches.[15] In any case, as an object for the improving attentions of science, engineering came a rather poor third after agriculture and medicine, in

England at any rate; in Louis XIV's France some attention was given to military engineering—and François Blondel was encouraged to write *L'Art de Jetter les Bombes*—as well as to the ornamental hydraulics at Versailles, but solely in response to orders from above.

It is true that, as Robert Boyle was fond of saying, scientists could learn from craftsmen, rather as Darwin was to learn from pigeon-fanciers. Experimental science had to master certain techniques: distillation, lens-grinding, glass-blowing, even turning and metal-working and the art of assay, though then as now the scientist often left this sort of thing to carefully supervised technicians. To that extent a minimum level of technological and engineering competence was necessary before serious experimental science could begin; a competence to be had far more readily in Florence, Paris, London, or Nuremberg about 1600 than it was in Aristotle's Athens. And engineers, like dyers and gaugers, suggested problems that demanded solution. So Galileo in the *Discourses* remarks on the mechanical wonders accomplished in the Arsenal at Venice, which leads him on to discuss the theory of structures; and later in the same book the observation that water in a suction-pump will rise no more than 30 feet is brought into a discussion of the vacuum. It was an observation that led through the barometer to Boyle's pneumatics and so back through the steam-pump to the mine again. But such few examples have been vastly overworked. In fact, the scientist and the engineer rarely coincided, for each preserved his own autonomy, the one of science, the other of craft. By the end of the seventeenth century, indeed, the scientist was already beginning to assume a position of conscious superiority to the "rude mechanicals"; he felt he knew enough to tell the engineer why his surveys were bad, why his estimates of water-delivery proved wrong, and why his vaults sometimes collapsed. But the corrections to customary practice—if such they were—that the scientist could suggest were as yet few, and were to remain few until the whole nature of technology had begun to change through the advent of new materials and a new source of energy.

REFERENCES

[1] See A. R. Hall, *Ballistics in the Seventeenth Century* (Cambridge, 1952).

[2] Of course it should be added that Newton was easily able to correct this error when it was pointed out to him, as he did in the second edition. It remains true, however, that some rather obvious physical effects of concern to the engineer were surprisingly resistant to theoretical analysis; see C. Truesdell, "A Program towards Rediscovering the Rational Mechanics of the Age of Reason," *Archive for History of Exact Sciences*, I (1960), pp. 3-36.

[3] Cf. *Horologium Oscillatorium*, 1673. Huygens at first believed that (theoretically correct) cycloidal motion was essential to give the pendulum time-

keeping accuracy; he had no part in the development of escapements (beginning with anchor, of William Clements, about 1671), though he did (like Hooke) also invent a spring-controlled balancewheel escapement. No scientist paid any serious attention to temperature compensation, which was dealt with by the clockmakers.

[4] English translation by Henry Crew and A. de Salvio, 1914 (and Dover reprint).

[5] The Marquess of Lansdowne, *The Double-Bottom Ship of Sir William Petty* (Oxford, Roxburghe Club, 1931).

[6] See pp. 381 ff.

[7] Because Galileo placed the neutral axis at the base of the beam; apart from this assumption his geometry is correct.

[8] Georgius Agricola, *De re metallica* (1556); Eng. trans. by Herbert Hoover and Lou Henry Hoover (1912; Dover reprint, 1950). Some further machines in Vannoccio Biringuccio, *Pirotechnia* (1540), Eng. trans. by Cyril Stanley Smith and Martha Teach Gnudi (1943; Basic Books, 1959) and other metallurgical authors (for bibliography see C. S. Smith in Singer *et al.*, *A History of Technology*, vol. III). The chief writers on machinery, Ramelli, Besson, Zonca, Veranzio, etc., are listed in William Barclay Parsons, *Engineers and Engineering in the Renaissance* (Baltimore, 1939), p. 118, in which also some features of their books are discussed.

[9] Cf. Parsons, *op. cit.*, pp. 438, 454.

[10] Cf. Maurice Daumas, *Les Instruments Scientifiques aux XVII^e et XVIII^e Siècles* (Paris, 1953); Edmond R. Kiely, *Surveying Instruments* (New York, 1947); E. G. R. Taylor, *The Mathematical Practitioners of Tudor and Stuart England* (Cambridge, 1954).

[11] The first surveying level with telescope, cross-wires, and bubble that I have seen was made by John Rowley and dated 1703; but there may be earlier ones extant. Huygens considered the use of such instruments for accurate survey many years before.

[12] Described in *New Experiments Physico-Mechanical touching the Spring of the Air and its Effects* (1660). In another book, *The Usefulness of Experimental Natural Philosophy* (Tome II, 1671), Boyle specifically mentions the instrument-making crafts as ones that had profited from scientific discoveries, giving as instances the telescope and microscope and the fact "that we daily see the shops of clockmakers and watchmakers more and more furnished with those useful instruments, pendulum clocks, as they now are called, which but a few years ago were brought into request by that ingenious gentleman, who discovered the new planet about Saturn" (*Works*, ed. T. Birch, London, 1772, III, p. 399).

[13] There is, however, almost a century's interval between these two machines, the development of the dividing-engine beginning c. 1750.

[14] Many scientists gave particular instructions or general directions to craftsmen about the construction of particular instruments; for one such "collaboration" see Henry W. Robinson and Walter Adams (eds.), *The Diary of Robert Hooke* (London, 1935), Index *s.v.* "Tompion."

[15] On this question in general see Robert K. Merton, "Science, Technology and Society in Seventeenth Century England," *Osiris*, IV, 1938—who inclines to the view that science was closely linked to technology—and G. N. Clark, *Science and Social Welfare in the Age of Newton* (Oxford, 1949), who suggests many reasons for not supposing either one to stand in a causal relation to the other. The contemporary attitude is well depicted in Boyle's *Usefulness* (above, n. 12).

The Technological Revolution: Notes on the Relationship of Technology, Science, and Culture

PETER F. DRUCKER*

THE STANDARD ANSWER to the question: What brought about the explosive change in the human condition these last two hundred years, is "The Progress of Science." This paper enters a demurrer. It argues that the right answer is more likely: "A fundamental change in the concept of technology." Central to this was the re-ordering of old technologies into systematic public disciplines with their own conceptual equipment, e. g., the "differential diagnosis" of nineteenth century medicine. In the century between 1750 and 1857 the three main technologies of Man—Agriculture, the Mechanical Arts (today's Engineering), and Medicine—went in rapid succession through this process, which resulted almost immediately in an agricultural, an industrial, and a medical "revolution" respectively.

This process owed little or nothing to the new knowledge of contemporary science. In fact, in every technology the practice with its rules of thumb, was far ahead of science. Technology therefore became the spur to science; it took, for instance, 75 years until Clausius and Kelvin could give a scientific formulation to the thermodynamic behavior of Watt's steam engine. Science could indeed have had no impact on the Technological Revolution until the transformation from craft to technological discipline had first been completed.

But technology had an immediate impact on science which was transformed by the emergence of systematic technology. The change was the most fundamental one—a change in science's own definition and image of itself. From being "natural philosophy" science became a social institution. The words in which science defined itself remained unchanged: "the systematic search for rational knowledge." But "knowledge" changed its meaning from being "understanding," i. e., focused on man's mind, to being "control," i. e., focused on appli-

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cation in and through technology. Instead of raising, as science had always done, fundamental problems of metaphysics, it came to raise, as it rarely did before, fundamental social and political problems.

It would be claiming too much to say that technology established itself as the paramount power over science. But it was technology that built the future home, took out the marriage license, and hurried a rather reluctant science through the ceremony. And it is technology that gives the union of the two its character; it is a coupling of science *to* technology, rather than a coupling of science and technology.

The evidence indicates that the key to this change lies in new basic concepts regarding technology, that is, in a genuine Technological Revolution with its own causes and its own dynamics.

I

Of all major technologies medicine alone has been taught systematically for any length of time. An unbroken line leads back for 1000 years, from the medical school of today to the medical schools of the Arab Caliphates. The trail, though partly overgrown, goes back, another 1400 years, through the School of Alexandria to Hippocrates. From the beginning medical schools taught both theoretical knowledge and clinical practice, engaged simultaneously therefore in "science" and "technology." Unlike any other technologist in the West, the medical practitioner has continuously enjoyed social esteem and position.

Yet, until very late—1850 or thereabouts—there was no organized or predictable relationship between scientific knowledge and medical practice. The one major contribution to health care which the West made in the Middle Ages was the invention of spectacles. The generally accepted date is 1286; by 1290 the use of eyeglasses is fully documented.[1] This invention was, almost certainly, based directly upon brand-new scientific knowledge, most probably on Roger Bacon's optical experiments. Yet Bacon was still alive when spectacles came in—he died in 1294. Until the nineteenth century there is no other example of such all but instantaneous translation of new scientific knowledge into technology—least of all in medicine. Yet Galen's theory of vision which ruled out any mechanical correction was taught in the medical schools until 1700.[2]

Four hundred years later, in the Age of Galileo, medicine took its next big step—Harvey's discovery of the circulation of the blood, the first major new knowledge since the ancients. Another hundred years, and Jenner's smallpox vaccination brought both the first specific treatment and the first prevention of a major disease.

Harvey's findings disproved every single one of the theoretical assumptions that underlay the old clinical practice of bleeding. By 1700 Harvey's findings were taught in every medical school and repeated in every medical text. Yet bleeding remained the core of medical practice and a universal panacea for another hundred years, and was still applied liberally around 1850.[3] What killed it finally was not scientific knowledge–available and accepted for 200 years–but clinical observation.

In contrast to Harvey, Jenner's achievement was essentially technological and without any basis in theory. It is perhaps the greatest feat of clinical observation. Smallpox vaccination had hard sledding–it was, after all, a foolhardy thing deliberately to give oneself the dreaded pox. But what no one seemed to pay any attention to, was the complete incompatibility of Jenner's treatment with any biological or medical theory of the time, or of any time thereafter until Pasteur, 100 years later. That no one, apparently, saw fit to try explaining vaccination or to study the phenomenon of immunity appears to us strange enough. But how can one explain that the same doctors who practiced vaccination, for a century continued to teach theories which vaccination had rendered absurd?

The only explanation is that science and technology were not seen as having anything to do with one another. To us it is commonplace that scientific knowledge is being translated into technology, and vice versa. This assumption explains the violence of the arguments regarding the historical relationship between science and the "useful arts." But the assumptions of the debate are invalid: the presence of a tie proves as little as its absence–it is our age, not the past, which presumes consistency between theory and practice.

The basic difference was not in the content but in the focus of the two areas. "Science" was a branch of philosophy, concerned with understanding. Its object was to elevate the human mind. It was misuse and degradation of science to use it–Plato's famous argument. Technology, on the other hand, was focused on use. Its object was increase of the human capacity to do. Science dealt with the most general, technologies with the most concrete. Any resemblances between the two was "purely coincidental."[4]

II

There are no hard and fast dates for a major change in an attitude, a world view. And the Technological Revolution was nothing less. We do know, however, that it occurred within the half century 1720

to 1770—the half century that separates Newton from Benjamin Franklin.

Few people today realize that Swift's famous encomium on the man who makes two blades grow where one grew before, was not in praise of the scientists. On the contrary, it was the final, crushing argument in a biting attack on them, and especially against the august Royal Society. It was meant to extol the sanity and benefits of non-scientific technology against the arrogant sterility of an idle enquiry into nature concerned with understanding; this is against Newtonian Science, for Swift was, as always, on the unpopular side. But his basic assumption—that science and application were radically different and worlds apart—was clearly the prevailing one in the opening decades of the eighteenth century. No one scientist spoke out against the weirdest technological "projects" of the South Sea Bubble of 1720, even though their theoretical infeasibility must have been obvious to them. Many, Sir Isaac Newton taking the lead, invested heavily in them.[5] And while Newton, as Master of the Royal Mint, reformed its business practices, he did not much bother with its technology.

Fifty years later, around 1770, Dr. Franklin is the "philosopher" par excellence and the West's scientific lion. Franklin though a first-rate scientist, owed his fame to his achievements as a technologist—"artisan" in eighteenth century parlance. He was a brilliant gadgeteer, as witness Franklin stove and bifocals. Of his major scientific exploits, one—the investigation of atmospheric electricity—was immediately turned into useful application: the lightning rod. Another, his pioneering work in oceanography with its discovery of the Gulf stream, was undertaken for the express purpose of application, viz., to speed up the transatlantic mail service. Yet the scientists hailed Franklin as enthusiastically as did the general public.

In the fifty years between 1720 and 1770—not a particularly distinguished period in the history of science, by the way—a fundamental change in the attitude towards technology, both of laity and of scientists, must have taken place. One indication is the change in English attitude towards patents. During the South Sea Bubble they were still unpopular and attacked as "monopolies." They were still given to political favorites rather than to an inventor. By 1775 when Watt obtained his patent, they had become the accepted means of encouraging and rewarding technological progress.

We know in detail what happened to technology in the period which includes both the Agricultural Revolution and the opening of the Industrial Revolution. Technology as we know it today, that is, systematic, organized work on the material tools of man, was born

then. It was produced by collecting and organizing existing knowledge, by applying it systematically, and by publishing it. Of these steps the last one was both the most novel—craft skill was not for nothing called a "mystery"—and the most important.

The immediate effect of the emergence of technology was not only rapid technological progress: it was the establishment of technologies as systematic disciplines to be taught and learned and, finally, the re-orientation of science towards feeding these new disciplines of technological application.

Agriculture [6] and the mechanical arts [7] changed at the same time, though independently.

Beginning with such men as Jethro Tull and his systematic work on horse-drawn cultivating machines in the early years of the seventeenth century and culminating towards its end in Coke of Holkham's work on balanced large-scale farming and selective live-stock breeding, agriculture changed from a "way of life" into an "industry." Yet this work would have had little impact but for the systematic publication of the new approach, especially by Arthur Young. This assured both rapid adoption and continuing further work. As a result, yields doubled while manpower needs were cut in half—which alone made possible that large-scale shift of labor from the land into the city and from producing food to consuming food on which the Industrial Revolution depended.

Around 1780, Albrecht Thaer in Germany, an enthusiastic follower of the English, founded the first agricultural college—a college not of "farming" but of "agriculture." This in turn, still in Thaer's lifetime, produced the first, specifically application-focused new knowledge, namely, Liebig's work on the nutrition of plants, and the first science-based industry, fertilizer.

The conversion of the mechanical arts into a technology followed the same sequence and a similar time table. The hundred years between the 1714 offer of the famous £20,000 prize for a reliable chronometer and Eli Whitney's standardization of parts was, of course, the great age of mechanical invention—of the machine tools, of the prime movers, and of industrial organization. Technical training, though not yet in systematic form, began with the founding of the *École des Ponts et Chaussées* in 1747. Codification and publication in organized form goes back to Diderot's *Encyclopédie*, the first volume of which appeared in 1750. In 1776—that miracle year that brought the Declaration of Independence, *The Wealth of Nations*, Blackstone's *Commentaries*, and Watt's first practical steam engine—the first modern technical university opened: the *Bergakademie* (Mining Academy) in Freiberg,

Saxony. Significantly enough, one of the reasons for its establishment was the need for technically trained managers created by the increasing use of the Newcomen steam engine, especially in deep-level coal mining.

In 1794, with the establishment of the *École Polytechnique* in Paris, the profession of engineer was established. And again, within a generation, we see a re-orientation of the physical sciences—organic chemistry and electricity begin their scientific career, being simultaneously "sciences" and "technologies." Liebig, Woehler, Faraday, Henry, Maxwell were great scientists whose work was quickly applied by great inventors, designers, and industrial developers.

Only medicine, of the major technologies, did not make the transition in the eighteenth century. The attempt was made—by the Dutchman Gerhard Van Swieten,[8] not only a great physician but politically powerful as advisor to the Hapsburg Court. Van Swieten attempted to marry the clinical practice which his teacher Boerhaave had started at Leyden around 1700, with the new scientific methods of such men as the Paduan Morgagni whose *Pathological Anatomy*[9] (1761) first treated diseases as afflictions of an organ rather than as "humours." But—a lesson one should not forget—the very fact that medicine (or rather, something by that name) was already respectable and organized as an academic faculty defeated the attempt. Vienna relapsed into medical scholasticism as soon as Van Swieten and his backer, the Emperor Joseph II, died.

It was only after the French Revolution had abolished all medical schools and medical societies that a real change could be effected. Then another court physician, Corvisart, Napoleon's doctor, accomplished, in Paris around 1820, what Van Swieten had failed in. Even then opposition to the scientific approach remained powerful enough to drive Semmelweis out of Vienna and into exile when he found, around 1840, that traditional medical practices were responsible for lying-in fever with its ghastly death toll. Not until 1850, with the emergence of the "modern" medical school in Paris, Vienna, and Wuerzburg, did medicine become a genuine technology and an organized discipline.

This too happened, however, without benefit of science. What was codified and organized was primarily old knowledge, acquired in practice. Immediately *after* the re-orientation of the practice of medicine, the great medical scientists appeared—Claude Bernard, Pasteur, Lister, Koch. And they were all application-focused, all driven by a desire to do, rather than by a desire to know.

We know the results of the Technological Revolution, and its impacts. We know that, contrary to Malthus, food supply in the

last two hundred years has risen a good deal more than an exploding human population. We know that the average life-span of man a hundred fifty years ago, was still close to the "natural life span": the 25 years or so needed for the physical reproduction of the species. In the most highly developed and prosperous areas, it has almost tripled. And we know the transformation of our lives through the mechanical technologies, their potential, and their dangers.

Most of us also know that the Technological Revolution has resulted in something even more unprecedented: a common world civilization. It is corroding and dissolving history, tradition, culture, and values throughout the world, no matter how old, how highly developed, how deeply cherished and loved.

And underlying this is a change in the meaning and nature of "knowledge" and of our attitude to it. Perhaps one way of saying this is that the non-Western world does not want Western science primarily because it wants better understanding. It wants Western science because it wants technology and its fruits. It wants control, not understanding. The story of Japan's "Westernization" between 1867 and her emergence as a "modern nation" in the Chinese War of 1894, is the classical, as it is the earliest, example.[10]

But this means that the Technological Revolution endowed technology with a power which none of the "useful arts"—whether agricultural, mechanical, or medical—had ever had before: impact on man's mind. Previously, the "useful arts" had to do only with how man lives and dies, how he works, plays, eats, and fights. How and what he thinks, how he sees the world and himself in it, his beliefs and values, lay elsewhere—in religion, in philosophy, in the arts, in science. To use technological means to affect these areas was traditionally "magic"—considered at least evil, if not asinine to boot.

With the Technological Revolution, however, application and cognition, matter and the mind, tool and purpose, knowledge and control have come together for better or worse.

III

There is only one thing we do not know about the Technological Revolution—but it is essential: What happened to bring about the basic change in attitudes beliefs, and values which released it? "Scientific progress," I have tried to show, had little to do with it. But how responsible was the great change in world outlook which, a century earlier, had brought about the great Scientific Revolution? What part did the rising capitalism play? And what was the part of the new,

centralized national state with its mercantilistic policies on trade and industry and its bureaucratic obsession with written, systematic, rational procedures everywhere? (After all, the eighteenth century codified the laws as it codified the useful or applied arts.) Or do we have to do here with a process, the dynamics of which lie in technology? Is it the "progress of technology" which piled up to the point when it suddenly turned things upside down, so that the "control" which nature had always exercised over man now became, at least potentially, "control" which man exercises over nature?

This should be, I submit, a central question both for the general historian and for the historian of technology.

For the first, the Technological Revolution marks one of the great turning points—whether intellectually, politically, culturally, or economically. In all four areas the traditional—and always unsuccessful—drives of systems, powers, and religions for world domination is replaced by a new and highly successful world-imperialism, that of technology. Within a hundred years, it penetrates everywhere and puts, by 1900, the symbol of its sovereignty, the steam engine, even into the Dalai Lama's palace in Lhasa.

For the historian of technology, the Technological Revolution is not only the cataclysmic event within his chosen field; it is the point at which such a field as "technology" emerges. Up to that point there is, of course, a long and exciting history of crafts and tools, artifacts and mechanical ingenuity, slow, painful advances and sudden, rapid diffusion. But only the historian, endowed with hindsight, sees this as "technology," and as belonging together. To contemporaries, these were separate things, each belonging to its own sphere, application, and way of life.

Neither the general historian nor the historian of technology has yet, however, concerned himself much with the Technological Revolution. The first—if he sees it at all—dismisses technology as the bastard child of science. The only general historian of the first rank (excepting only that keen connoisseur of techniques and tools, Herodotus) who devotes time and attention to technology, its role and impact is, to my knowledge, Franz Schnabel.[11] That Schnabel taught history at a technical university (Karlsruhe) may explain his interest. The historians of technology, for their part, tend to be historians of materials, tools, and techniques rather than historians of technology. The rare exceptions tend to be non-technologists such as Lewis Mumford or Roger Burlingame who, understandably, are concerned more with the impact of technology on society and culture than with the development and dynamics of technology itself.

Yet technology is important today precisely because it unites both the universe of doing and that of knowing, connects both the intellectual and the natural histories of man. How it came thus to be in the center—when it always before had been scattered around the periphery—has yet to be probed, thought through, and reported.

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[2] It was among the great Boorhaave's many "firsts" to have taught the first course in opthalmology and to examine in it actual eyes—in 1708 in Leyden. Newton's *Optics* was the acknowledged inspiration. (See George Sarton, "The History of Medicine versus the History of Art," *Bulletin of the History of Medicine*, 10 (1941), pp. 123-35.)

[3] Bleeding actually reached a peak in the 1820's when it was touted as the universal remedy by no less an authority than Broussais, the most famous professor at the Paris Academy of Medicine. According to Henry E. Sigerist (*The Great Doctors*, New York, 1933), it became so popular that in the one year, 1827, 33 million leeches were imported into France.

[4] There was, to be sure, one famous dissent, one important and highly effective approach to science as a means to doing and as a foundation for technology. Its greatest spokesman was St. Bonaventure, the thirteenth century antiphonist to St. Thomas Aquinas (see especially St. Bonaventura's *The Reduction of all Arts to Theology*). A hundred years earlier the dissenters actually dominated in the twelfth century Platonism of the theologian—technologist schools of St. Victoire and Chartres, builders alike of mysticism and of the great cathedrals. On this see Charles Homer Haskin, *The Renaissance of the 12th century* (Cambridge, Mass., 1927); Otto von Simson *The Gothic Cathedral* (New York, 1956); and *Abbot Suger and the Cathedral Church of St. Denis*, edited by Erwin Panofsky (Princeton, 1946).

The dissenters did not, of course, see material technology as the end of knowledge; rational knowledge was a means towards the knowledge of God or at least His glorification. But knowledge, once its purpose was application, immediately focused on material technology and purely worldly ends—as St. Bernard pointed out in his famous attack on Suger's "technocracy" as early as 1127.

The dissent never died down completely. But after the Aristotelian triumph of the thirteenth century, it did not again become respectable, let alone dominant until the advent of Romantic Natural Philosophy in the early nineteenth century, well *after* the Technological Revolution and actually its first (and so far only) literary offspring. It is well known that there was the closest connection between the Romantics—with Novalis their greatest poet, and with Schelling their official philosopher—and the first major discipline which, from its inception, was always both science and technology: organic chemistry. Less well known is the fact that the Romantic movement, its philosophers, writers, and statesmen came largely out of the first technical university, the Mining Academy in Freiberg (Saxony) that had been founded in 1776.

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[9] This is the name commonly used for the work. Its actual title was *De Sedibus et causis morborum per anatomen indigatis*; the first English translation appeared in 1769 under the title *The Seats and Causes of Diseases Investigated by Anatomy*.

[10] This is brought out most clearly in William Lockwood, *The Economic Development of Japan, 1868-1938* (Princeton, 1954).

[11] Franz Schnabel, *Deutsche Geschichte im 19. Jahrhundert* (4 vols., Freiburg, 1929-1937); the discussion of technology and medicine is found chiefly in Vol. III.

Chemistry, Chemical Technology, and Scientific Progress

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As A SCIENCE, chemistry is of very recent development. The technology of chemistry, however, is as ancient as man. For almost his entire existence as a species man has used chemical processes and enjoyed the products of chemical change, even though he had no concept of the forces he was utilizing. During all this early period the development of chemical processes was extremely slow. Undoubtedly there were prehistoric periods of breakthrough, such as must have occurred when fire was first controlled, and then there may have been a sudden brief advance. However, innate conservatism and the tendency to conceal trade secrets and hand them down within families or guilds prevented any marked change in chemical methods over the centuries.

By the time civilizations emerged, technology had become well standardized. In Mesopotamia, for instance, it had reached a high level.[1] The artisans who applied technological methods were too busy with their practical affairs to spend time on theoretical speculations, however. There seems to be no evidence that they tried to explain why their processes worked. This was the realm of the priestly class. In the temples men had time and the inclination to wonder why the world operated as it did. They were not limited by hard, observed facts in their thinking, for they did not work with physical materials. There is little wonder that most of their theories tended to the mythological and supernatural. While contact between the artisan and the philosopher was lacking, scientific and technical progress was slow.

A consideration of the history of chemistry helps to show both the effects of the separation of theory and practice and the results of their union. The conditions which favored the beginning of chemistry as a science came from the merging of two major currents in Hellenistic Alexandria. The Greek philosophers of the classical period took

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all knowledge for their province. Their cosmological speculations involved many theories of the composition of matter and of how matter could change. These have always been the basic problems of chemistry, and to this extent the philosophers were thinking chemically. However, their widely ranging theories involved most other sciences as well, and they made few if any actual applications of their ideas.

In the meantime, the artisans of the Near East had been perfecting their technological skills and had developed a degree of specialization which resulted in a true chemical technology among the metallurgists, the dyers, and workers in similar fields. In Alexandria for the first time the technologists turned to the philosophers to gain an understanding of their crafts. They found that they could use the Aristotelian theory of matter and its changes to explain the actual changes which they observed in their own workshops. It was this union of theory with technology which gave birth to the science of chemistry, although this new development has come to be known by the slightly disreputable name of alchemy among historians of science.

In its initial stages, alchemy was a true science. The artisans operated on a theory, that of the interconvertibility of matter, which satisfactorily explained many of the changes which they saw when they prepared alloys, changed the color of metals, and made cheap imitations of expensive substances that seemed to be as good as the original materials. Their theories enabled them to predict when they should apply gentle heat in their processes and when more violent methods were needed; or what reagents would produce desired color changes, which were an important part of their art,[2] and how to prepare these reagents. The form of their apparatus, the number of distillations they made in preparing reagents—all these were determined on a theoretical basis. At the same time they watched for new materials which could be put to use in their workshops. These first two or three centuries of our era when theory and practice in chemistry first came together was actually one of the most important periods in the history of the science.

Unfortunately, there were limits to what could be done with the theory and information available to the Alexandrian chemists. When these limits were reached, progress began to slow down. In Alexandria a great number of mystically minded philosophers surrounded the practical artisans. These men were only too happy to seize upon the alchemical ideas and convert them into a maze of allegorical speculations. Much of alchemical theory, with its emphasis upon the perfection of metals to gold, was ideally adapted to represent the always popular concept of the perfection of the human soul. As soon as

philosophers who had no contact with the laboratory took over these ideas, their imaginations were given free rein and theoretical speculation moved away from practical technology. Progress in a chemical sense stopped. Thus the later Byzantine artisans contributed nothing to chemistry, either as a science or an art.

Luckily, the practical science of alchemy was kept alive, at least in manuscript form, by the activities of the Nestorian scholars of Asia Minor who broke off from the Orthodox Church and established their own schools, in which they taught Greek science in translations into Syriac, their official language. These schools served as a bridge by which the Greek writings reached a new people ready for further advance, the Arabs, whose golden age of science came in the tenth and eleventh centuries.

In the Arabic world again, theory and practice merged, and chemistry advanced in another leap. The Arabs had inherited much of the Mesopotamian technology,[3] and their philosophers eagerly seized upon Greek cosmological ideas. In the practical Arabic alchemists these currents combined with results much the same as in Alexandria.

Guided by the theory of the four elements of Aristotle and their own modification of it, the sulfur-mercury theory of metal composition, the alchemists among the sect of the Isma'iliya who compiled the vast body of works issued under the name of Jabir ibn Hayyan, tried to isolate such bodies as "cold" by repeated distillations. While they probably did not carry out the more than seven hundred distillations which theory required of them, they did discover a great deal about the practical art of distillation, and they applied their knowledge in the preparation of many plant and animal products. They worked out a theory of a definite ratio of qualities in material substances, and from this "Theory of the Balance"[4] they arrived at some sense of the importance of a quantitative method.[5] Their studies led to the isolation of new materials, such as the caustic alkalis and sal ammoniac.

During the early Middle Ages western Europeans had abandoned the search for theoretical concepts, though their technology continued to advance slowly and in largely unrecorded ways. By the twelfth century, however, their interest in such concepts revived, and they turned to the only source of information available to them. As it happened, this was the period when Arabic alchemy, like its Hellenistic predecessor, had almost reached the limits of its possibilities and was declining into mysticism and allegory. Its achievements were preserved in the translations which at this time began to be made in Spain and Sicily, and the technologists of Europe found a new basis for the understanding of their processes.

The impact of this new meeting of theory and practice is clearly seen in the important writings of the pseudo-Geber in the fourteenth century. He was apparently a practicing Spanish alchemist who was well acquainted with the laboratory and the behavior of metals and reagents; at the same time he knew the Aristotelian theories and the sulfur-mercury concept of metallic composition, and his works reveal a clear utilization of these ideas to explain what he was doing and why.

After the fourteenth century theory and practice in chemistry were never again separated in western Europe. There was a continuous development, in which physicians, apothecaries, and technologists shared, and nearly every great man in the history of chemical thought from the fifteenth to the seventeenth century was also noted for practical discovery. Thus Paracelsus and Van Helmont proposed significant theories and did equally important technical work. Libavius, Glauber, Agricola, Biringuccio, and Ercker were chiefly interested in technology, but they helped to advance the quantitative approach to chemistry; by their descriptions and discoveries they systematized alchemy and converted it into a true science of chemistry.

The periods of great chemical progress now came more closely together. There were still variations in the rate of advance, however, largely due to overemphasis on one side of chemistry. While it was necessary to bring the theories of matter into the framework of the atomic theory, the period in which this was done was one of great confusion. It terminated in the phlogiston theory, the first great generalization in chemistry, and then again the technologist could speculate freely on the causes of his processes. Especially in France in the eighteenth century the technologists and philosophers merged into a coherent group.[6]

The tradition of the chemist who was also a technologist reached its climax in Lavoisier, who not only founded modern theoretical chemistry but was also an important investigator of the manufacture of saltpeter, an innovator in agricultural chemistry, and an active contributor to the understanding of the scientific basis of national economy. Nor was the great period of French chemistry solely due to Lavoisier. Berthollet was as well known for his work on dyeing and bleaching as for his theories, and Gay-Lussac improved sulfuric acid manufacture. The stresses of the French Revolution and the Napoleonic wars led to such developments as the Leblanc soda process. At the beginning of the nineteenth century France led the world in her chemistry. During the century this situation changed sharply, and the reasons seem to lie in her method of developing and applying chemistry.

France concentrated all her scientific activity in Paris and gave the positions of responsibility to a few outstanding men. Even such able chemists as Gerhardt and Laurent had to work under unfavorable conditions because they had earned the enmity of the all-important Dumas. There was almost no method for training a large group of chemists for the increasingly important chemical industries, and so a slow and steady decline in importance set in. The leadership in chemistry passed to Germany.

The dye industry gave Germany her chance. Although this had first been organized in England and France, neither of these countries foresaw the possibilities in its development.[7] The German dye works of the second half of the nineteenth century became the model upon which all chemical industry is now built. As they grew in importance, they called for more and more technically trained chemists, and the universities developed along with them to supply the need. University professors became consultants, improving both their scientific and economic positions. German science assumed world leadership. There has never been a clearer case of the value of cooperation between theoretical and applied chemistry.

The examples cited in this brief survey certainly could be greatly extended, and similar developments could be cited in other fields of science. However, enough has probably been said to indicate the great importance of maintaining a balance between the so-called "pure" and "applied" branches of science. When separation between them occurs, there is always the danger that one or the other branch may become dominant. As history shows, the marked predominance of either branch slows scientific progress.

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The Interaction of Science and Practice in the History of Metallurgy

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A QUICK LOOK at metallurgy might suggest that its development owed very little to science. An immense and important body of knowledge on the useful alloys had been discovered long before anything approaching modern science existed, and even today the theoretical metallurgist is principally engaged in explaining phenomena that have been known for over 4000 years. Moreover, a majority of workers in metal have unthinkingly followed traditional methods with very little desire to apply the science of their day or to use their practical knowledge as an incentive to theoretical thinking. Yet, although he was motivated only by the desire to make something useful or beautiful, or merely profitable, the artisan who experimented with different materials to select the best for his purpose was as much a contributor to the development of science, was as much of a scientist, as the observational astronomer or the classifying mineralogist or naturalist. Theoretical understanding of the nature of metals and their reactions has rarely led directly to new discoveries, simply because empirical experiment had discovered them in advance of theory.

Any branch of science must draw from a pre-scientific period in which facts are observed and recorded before they can be put into a theoretical framework. Once theory exists, it will (to a modern mind at least) demand new experiments, but before good theory has been formulated, intelligent observation alone is king, and any motivation for handling matter with open curiosity will stimulate the gathering of information. To quote Robert Mehl, "The metallurgical plant is a living museum of entrancing phenomena."[1]

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Early Knowledge of Metals

There is, of course, no record of the manner or background of the initial discovery of the utility of native metals, or of the more important discovery that metals could be obtained by putting some heavy and colored rocky substances into a carbonaceous fire, but it happened well over 5000 years ago. Three and a half millennia ago, most of the common metals which are capable of being reduced from their ores by this simple technique had been used in nearly pure or in alloyed forms. The most critical step was the development of means of producing a high enough temperature to melt copper, perhaps in a wind-swept campfire or a potter's kiln.

At the height of the great Mesopotamian civilizations examples of almost all generally recognized metallurgical phenomena had been discovered and put to good use—oxidation and reduction; the separation of metals by using the immiscibility of specific metals, slags, and sulphides; work hardening and annealing; changes of melting point and hardness by alloying. Not much later the weldability of iron and its immensely important allotropic transformation were discovered and utilized. The body of knowledge quickly and impressively spread almost to the limit of what could be found with charcoal as reducing agent and fuel. It is doubtful if this owed anything to science.

More than anyone else the metallurgist would be familiar with the Aristotelian elements, for earth, air, fire, and water were the very basis of his daily activities. But, however much one may admire the towering intellectual achievements of the Greek philosophers, from the narrow viewpoint of the metallurgist their schemes merely partially classified some practical observations and were nearly useless as guides for extending knowledge or for devising new processes. It was a craftsman's need, associated with curiosity more than philosophy, that led to the trials that gave the refinements of tin control in bronze, the addition of lead, and the discovery of a range of solders for base and precious metals, to mention but a few of the early discoveries.

Although there were other areas in which simple experiments might have collected a body of empirical information, these were neglected despite their intellectual interest. In the early compilations there is very little mechanics accompanying the information on materials, probably because the utility of materials for simple tools and ornament was more apparent than the potential value of machines. Relatively advanced metallurgy and ceramics remained alongside primitive mechanics until the eighteenth and nineteenth centuries.

The Intermediate Period

After the early growth of metallurgy there followed many centuries in which little new was discovered. Metals increased enormously in industrial importance. They were fashioned into innumerable works of art and objects of simple utility, and they became a cause and apparatus of wars. Their decay by corrosion or oxidation led to endless speculation by philosophers and alchemists. But with all this activity it was not until after the beginnings of recognizably modern science that the knowledge that had been obtained by the empirical studies of the earliest metallurgists was substantially extended.

Standards of literacy and librarianship have precluded the survival of any substantial description of the European methods of alloying and working of metals prior to the early twelfth century (the *Diversarum Artium Schedula* of Theophilus the monk) and it is not until the sixteenth century that there are detailed records of metallurgical practice in general. However, the fragmentary evidence surviving from earlier periods suggests that these books could have been written much earlier, for little had changed except the scale of operation. The liquation process for desilverizing copper was about the only basic innovation in non-ferrous metallurgy after the invention of brass in Roman times. In siderurgy a profound change had indeed occurred in the introduction of the blast furnace, cast iron, and the indirect method of wrought iron production, and here the literacy factor precluded adequate description until well into the eighteenth century.

In the history of chemistry prominence is properly given to alchemy. This arose from the well-known ability of metals to change character or color during various treatments, but there is little evidence that it ever helped to solve the problems of a metallurgist. Almost the only contribution of alchemists was the discovery of nitric acid and its use in parting. There is no reason to suspect any connection between the Arabs cherishing both alchemy and the "Damascus" sword, at the time the finest example of metallurgical skill.

Metallurgy and Science in the 17th and 18th Centuries

The writings of the best seventeenth century chemists contain many descriptions of facts well known to the metallurgist and assayer, cited as experimental support for a theory. Conversely, however, metallurgical books long remained without a shred of theoretical explanation of the processes described. Even the exquisite quantitative methods of the assayers were presented only as recipes.

Early in the eighteenth century, however, this began to change. The work of R. A. F. de Réaumur [2]—undertaken at the express request of the *Académie des Sciences* to benefit the French steel industry—is a masterly example of the application of theoretical principles (in this case the corpuscularism of Descartes) to a practical problem.

About this time metallurgy gave rise to one of the most fertile periods of chemistry. Stahl's theory of phlogiston was largely based on the oxidation of metals and their reduction, known and used quantitatively by metallurgists for centuries. Similarly, chemical thermodynamics had its beginnings in the tables prepared by Geoffroy in 1717 to show the relative affinity of substances for each other. Of the 76 pairs discussed by Geoffroy, 49 involve metals, and they are mostly the simple separations that had been used for centuries in smelting operations or in assaying. Note also that the realization that materials differed in their specific heats first arose from metallurgical experiments on quenching (Réaumur, 1722). It was a practical observation during the boring of cast guns that was the basis of Rumford's important studies of the mechanical nature of heat.

Beginning with J. A. Cramer's *Elementa Artis Docimasticae* (1739),[3] chemical theory begins to show its real value to the metallurgist. The selective qualities of various kinds of menstrua were related to affinities, and phlogiston appears in almost every section of this work, throwing light alike on the roasting and reduction of lead or zinc, the action of the black flux in assaying, and the nature of steel.

Metallurgists could hardly have avoided noticing the evolution of gas in many of their reactions but they seem to have been astonishingly blind to its significance. Eventually, however, the phenomenon of the effervescence of charcoal reacting with litharge caught the attention of Lavoisier and was actually the origin of that great chemist's work on combustion in the 1770's.[4]

The discovery of oxygen and the composition of water, with the establishment of the basis of modern chemistry, occurred at the very time of the greatest single contribution of chemistry to metallurgy—the identification of the material element, carbon, as responsible for the differences between cast iron, wrought iron, and steel. The relationship of this to the Chemical Revolution has not been studied in detail,[5] but in outline it is somewhat as follows. The superiority of the famous swords of Damascus and of the Franks and Vikings was related to a visible pattern that depended on the manner of manufacture and was to some extent a guarantee that the sword had been correctly processed. Although the secret of the Damascus blade was not understood in Europe until 1823, the nature of weld-textured swords and guns

was easy to discover, and European factories were established in the eighteenth century to duplicate the famous Turkish and Indian gun barrels. A descriptive paper by the manager of one of the factories, in 1773 incited a metallurgist, Sven Rinman, to a detailed study of the action of acids on iron and steel, in the course of which he showed that there was a real difference in composition between the different siderurgical products, and that the solution of cast iron in acid left behind a residue of a plumbago-like material, which he suspected had its origin in the charcoal fuel of the blast furnace. This observation, in turn, was picked up by the famous chemist Bergman, who, after quantitative studies, stated clearly in 1781 the essentially material distinction between iron and steel. His work was translated into French and in turn inspired a paper (1786) by Berthollet, Monge, and Vandemonde—a chemist and two mathematicians, all three among the greatest scientists of their day. With utmost clarity they show that elemental carbon was responsible for the difference, and they erred only in misunderstanding the nature of white cast iron.

Almost as important as the identification of hydrogen and oxygen in the establishment of the new chemistry was the identification of the double role of carbon in the reduction of calces. The work of Guyton de Morveau in this connection deserves study, for he was intimately aware of the whole range of metallurgical phenomena and had published a perceptive article [6] on the nature of steel shortly before the appearance of the *Méthode de nomenclature chimique* (1787), in which he collaborated with Lavoisier and which did so much to spread the latter's chemical system.

Metallurgy also had important interactions with physics in the eighteenth century, although understanding was slower in developing than in chemistry. In 1761 a French iron-master, Grignon, observed the crystallization of large masses of iron and revived the seventeenth century idea that geometric shapes of crystals were inevitably formed by the packing together of uniform parts, an idea that led to the mathematical crystallography of Haüy (1784). Thereafter, however, the very perfection and elegance of crystalline mathematics somehow prevented its application to metallurgical realities, and physicists developed no interest in the irregular and imperfect crystalline grains of most real matter until nearly the middle of the twentieth century.

The Nineteenth Century

The nineteenth century illustrates well the fundamental difference between the attitude of the technologist and that of a scientist. The

technologist is concerned with complex materials in their actuality, and his knowledge will usually be empirically gathered, for it is likely to involve far more factors than can be treated exactly by the scientist.

The laws of fixed composition and of simple combining proportions in compounds, which were so admirably expressed in Dalton's atomic theory and led to the superb achievements of nineteenth century chemistry, were, from a metallurgist's viewpoint, temporarily unfortunate. Berthollet had rightly insisted that many compounds were *not* of fixed composition, but the advance of chemical theory required blindness to those things which did not fit.[7] The chemistry of compounds flourished, and atomic weights were established largely because chemists ignored the whole range of solid solutions that constitute the alloys, mattes, and slags of the metallurgist. Dalton's was a theory of the simple molecule, and the molecule dominated nineteenth century chemistry and physics, providing the basis of most theories of the behavior of solids. Though the concepts were basically correct in the case of gases, in most solids the molecule is the crystal, and it is incidental that the geometric requirements of the lattice often give a simplicity of composition in accord with that of a small molecule. When it was discovered that iron underwent transformation, it was easy to talk in terms of molecular allotropy and to feel no desire to study microscopically visible changes. There was, therefore, a long period when the structure of metals was developed by practical metallurgists with very little help indeed from the people who were leading the advances in pure science. The basic principles of polycrystallinity, of solid solutions, of crystalline transformation, of segregation, diffusion, and deformation all grew out of metallurgical studies. Both metallurgists and physicists failed to pick up the suggestion of the French elastician, Savart (1827), as to the true nature of most solid materials. The concern with crystal symmetry and with elasticity became so highly mathematical that slight departures from ideality could not be regarded as important. The parallel with the attitude of the theoretical chemists is quite close.

Metals were naturally involved intimately in the growing science of electricity. The discovery of thermoelectricity in 1822 led to a practical temperature measuring device sixty years later which had enormous scientific and industrial repercussions. The realization that copper had to be pure to be a good conductor of electricity came just at the time when electrolysis made its preparation commercially possible. Matthiessen's electrical conductivity measurements (*ca.* 1860) came at a critical time to influence metallurgists' thinking on the nature of alloys. Conversely the studies of mechanical properties of

metals and alloys that had been made earlier by the physicists Hooke, Musschenbroek, and, particularly, the neglected Achard (1788) attracted little interest in either scientific or industrial circles; such tests did not relate to the basic problems that were troubling scientists at the time, and the state of technology was such that individually useful alloys were already being used successfully and their absolute or comparative strengths were of little concern to practitioners.

The use of etching on iron meteorites in 1803 provides an interesting example of the cross-influence of one branch of science on another, for a direct path led from this to the beginning of microscopic metallography in the hands of Sorby in 1863. Here is an instance of an eye-opening technique rather than a mind-opening idea.[8]

Perhaps the greatest achievement of nineteenth century science was the thermodynamics of Gibbs. This was exploited by metallurgists in the form of the phase rule—but not, it must be noted, until after they had catalogued and puzzled over innumerable structures and changes of structure observed empirically under the microscope in alloys of various compositions and treatment. The simple phase rule not only enabled metallurgists quickly to understand their binary and ternary systems and to limit the range of the possible, but it also provided the background against which various transformations could be studied.

The innovations which marked the discontinuous stages of growth of the iron and steel industry—the introduction of the blast furnace and finery, of puddling, and of the Bessemer and open-hearth steelmaking processes—all owed almost nothing to the direct influence of science. Indeed, during the great eighteenth century industrial advances in England, English scientists were paying little attention to metals, and the scientific advances in France and Sweden brought scant immediate profits to their countries' industrialists. Although Bessemer remarked in his 1856 paper that he built his first converter with a view of testing practically a theory involving the reaction of carbon and oxygen, from his autobiography it is clear that his work was precipitated simply because he happened to note an unmelted shell on a pig of iron that had been superficially oxidized. However necessary theoretical understanding may be for final development, it is often a chance observation that suggests a new process, or even a new stage of science. The Pattinson process for desilverizing lead arose from a spilled crucible; the relationship between fracture and quality in "art castings of iron" led to Martens' microscopic work and to general acceptance of metallography; and it was the faint line of different texture left by a lathe tool on a gun forging which caused Tschernof to realize the importance of allotropy in the hardening of steel. Yet

growth is distinct from nucleation, and even if most important metallurgical events have been triggered serendipitously, their development, by the nineteenth century, has owed almost everything to the growing background of pure science.

The new metals, particularly aluminum, were the direct outgrowth of chemical science. It could hardly be otherwise, since it was science and not empiricism that gave rise to electrolysis and broke the age-old dependence of metallurgy on carbon as fuel and reducing agent. Similarly, it was a largely scientific study of carbonyls that led to the Mond nickel process. Furthermore, chemical analysis, whose role in nineteenth century metallurgy is hard to overestimate, repaid the contributions of the metallurgical assayer to the beginnings of analysis. The possibility of chemical analysis of ore and products not only extended enormously the range of useful raw materials, but it gave a degree of control that had previously been impossible and so enabled the enormous increase in scale that characterized nineteenth century metallurgy, even in those industries that continued to use processes that were centuries old. New analytical methods showed the existence of new elements and eventually made them available for alloying by the practical metallurgist. Every new method of analysis, whether the Bergmanian scheme at the end of the eighteenth century, the spectrograph in the mid-nineteenth, or the nuclear methods of the twentieth, has produced new grist for the metallurgist's mill and new means of studying his age-old problems.

Mechanical Metallurgy in the 19th Century

The older metals and alloys are all characterized by great tolerance of abuse and they were used for a wide range of applications. The newer alloys are designed for extreme and specialized service conditions and need a high degree of control at every stage in their production and use. This inevitably requires understanding of the scientific basis of the significant properties in order that they may be measured and adjusted.

Most engineering advances before the twentieth century were based on the utilization of the properties of materials that were readily available, although refinement occurred in use. (Printing is one of the more interesting cases: It seems to have begun using pewter as type-metal, but the cheaper and better lead base alloys were soon developed.) To a large extent the slowness in the development of special alloys was due to the fact that the engineer designed on the basis of his tests of materials available from the producers of metals and alloys in bulk,

who had little interest in changing established processes or compositions. The discovery of silicon steel by Robert Hadfield is a shining exception, for it was developed (by empirical trial) specifically to cut transformer losses in electrical distribution. High-speed steel resulted from a chance observation of an efficiency expert, not from a steelmaking metallurgist, and forecast the time early in the twentieth century when the metal consuming industries—the electrical industry being particularly notable—developed an interest in materials on a comparative basis and themselves did intensive work on those properties of alloys that were related to use. Most metallurgists prior to the twentieth century were employed in the primary smelting of metals; now they are more often concerned with the problems involved in the use of metallic materials.

Conclusion

Much of the change in metallurgy and the increase in scale and efficiency that occurred in the nineteenth century was a result of the application of science. Metallurgy has become truly an applied science. But, on closer inspection, it is apparent that science was more important is explaining processes that had been in use for long periods and in providing methods for the control of them than in directly promoting the invention of entirely new processes. Although all innovation must build on the platform of the past, the big changes in metallurgy have always had less of logical science in them than of inspiration, intelligent observation, deduction, and empirical experiment. Very rare are cases where theory preceded the reality. Certainly, with today's chemical theory, it would be simple to devise methods of smelting iron. But actually the experiments had preceded the theory, and charcoal was used for fuel and reductant long before the combined chemical and physical nature of its effect was understood, or even the chemical role of air suspected. Even today, chemical theory is not enough. A knowledge of complex properties of materials is as important as of the kinetics of reactions. In the development of the great majority of processes, the greatest task is the finding of materials of construction which will be compatible under the conditions encountered. Fundamental science cannot yet eliminate trial of refractories or effectively balance the economics of supplies of various sources of materials differing in impurity content. Moreover, science by its very nature cannot suggest objectives.

Because the laws that govern the behavior of metals are those of all matter, metallurgy is not itself a separate science. Broadly speaking,

engineering and structural materials can be divided into stone and ceramics, organic materials and plastics, and metals, each related to a particular kind of quantum-mechanical electron function; but metallurgy has developed a professional uniqueness mostly because of the economics of the natural occurrence and processing of metals, and there is a tendency at present for the scientific metallurgist to merge into an engineer concerned with the structure and properties of all materials. It is interesting to note that the principal contributions that have been made to pure science by metallurgists are in the fields of chemical analysis and crystallography, neither of which is related to the basis of metallicity.

The close contact between metallurgists and physicists that resulted from their common association on various wartime projects during World War II led to increased appreciation of each other's viewpoint and gave rise to a very fertile period in which the physicist's viewpoint basically affected the thinking of metallurgists and in which metallurgical facts were seen to be of interest to physicists. It may be that at the moment we are approaching another change in the relationship between science and metallurgy. The physicist has (although "in principle" only) explained most of the metallurgist's mysteries, and in doing so has laid the foundation for a truly applied science. Just as the analytical chemist in the nineteenth century showed the metallurgist the compositional reasons for the distinction between his various alloys and enabled him to control production, so in the twentieth century the physicist explains the reason for the various properties that have long been used and, through the improved instrumentation which is an outgrowth of physics, has enabled better control than ever before. Yet it will always be the function of the metallurgist to go beyond the scientific knowledge of the materials with which he is concerned and to adapt them to all kinds of conditions, including those imposed by economics and politics. The facts that he uncovers will in the future be of interest to science only if science becomes able to deal with reasonably complex things. In the past science has had its greatest, indeed its only, successes in dealing with things which can be isolated in thought and experiment to the point of extreme simplicity.

As metallurgy comes to conform more closely to the ideal applied science, it may perhaps become less exciting, for it will involve fewer glimpses of things beyond the frontiers of science. On the other hand, perhaps, if science as it grows develops methods for dealing communicably with complexity, the metallurgist's role (like that of the biologist) may well be to call attention to some areas in which more rigorous methods could profitably be employed.

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Science and Engineering in Agriculture: A Historical Perspective

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MODERN AGRICULTURE is involved in a veritable maelstrom of economic, scientific, and technological changes. The past sixty years have probably witnessed more changes than all previous centuries, and in some aspects the changes during the past five years surpass those during the previous fifty. Revolutionary changes have transformed agriculture in the advanced industrial countries from a way of life to a highly specialized, mechanized business with a heavy capital investment.[1] Yet, during the past three centuries, in Europe and America, technical changes in agriculture came by slow degrees and tended to lag behind urban technology.

Many factors have affected the rate of technical progress in agriculture. It is admittedly difficult to generalize concerning the large, heterogeneous group engaged in diverse agricultural pursuits. While some farmers in every age welcomed innovation, especially when it reduced physical labor, others clung tenaciously to the proven methods of their ancestors.[2] The fact that nearly all farming operations can be done by hand or with the aid of simple tools reduced the need to mechanize; and necessity is proverbially the mother of invention. Moreover, an ample labor force was usually available, and draft animals also provided meat, fibers, and hides.

The inadequacies of scientific data, reflected by the pseudo-scientific information presented in so-called books of husbandry, provoked a healthy scorn of book learning among practical farmers everywhere. "What has science done for agriculture?" queried a Scottish savant in an essay published in 1842.[3] Only recently and in a limited way, he observed, had attempts been made to apply science to agriculture; but "a great drawback to the advance of agricultural science is to be found in the lack of confidence in it of practical men. . . ." As to which of the newly invented farm implements "owe their origin to scientific investigation," the author confessed he knew of none.

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The generally limited capital available retarded the development and use of mechanized techniques in agriculture. The price of a steam engine, for example, was beyond the reach of most farmers; moreover, its seasonal use and the fact that it supplemented rather than replaced draft animals and hand labor meant that the labor force, draft animals, and the engine itself would be idle during parts of the year. Steam power, together with machines and the factory system, revolutionized industrial production, but made only a slight impression on agriculture. Even though isolated wealthy planters in England and the United States were operating steam engines for threshing, sawing wood, and grinding sugar cane before steam locomotives were used successfully on railroads, these agricultural engines were not adaptable for multi-purpose farm work [4] or for small farming units. Small farms and the typical fragmented fields of agrarian Europe limited the feasibility of mechanization.[5] Interestingly, in 1848 the Patent Commissioner, in prophetic recognition that new forms of power were the key to technological progress in agriculture, recommended that Congress offer a prize of $100,000, or perhaps $1,000,000, for a new type of motor in which some explosive substance in a closed cylinder would drive pistons attached to rods.[6]

A special problem of agricultural engineering is the difficulty of adapting machines to perform with precision on the highly variable organic materials; thus, devising a satisfactory milking machine long baffled inventive ingenuity. Equally difficult was the designing of machines to operate on soft or uneven terrain; McCormick and others who invented reapers encountered this perennial problem, as did the legion of inventors who devised seed drills, planters, and cultivators. In 1849 the United States granted twenty new patents involving seed planters in which the principal innovation was the use of springs "able to adjust their resistance to that of any given soil without yielding, but when they meet a firm resisting body, the points of the teeth yield and draw backwards, and the tooth thus slides over the obstruction." [7] In the same year, the Patent Commissioner reported that, against his better judgment, he had registered a patent for a cotton picker, the first such implement in Patent Office annals; but he warned that the invention is "of doubtful practicality" since not all bolls ripen simultaneously, and cotton is generally picked two or three times.

Bearing in mind these various factors which retarded, until relatively recently, technical progress in agriculture, we can turn back to the beginning of modern times to trace the interrelations of science and engineering in overcoming those hindrances.

Early Efforts to Apply Scientific Knowledge to Technical Problems in Agriculture

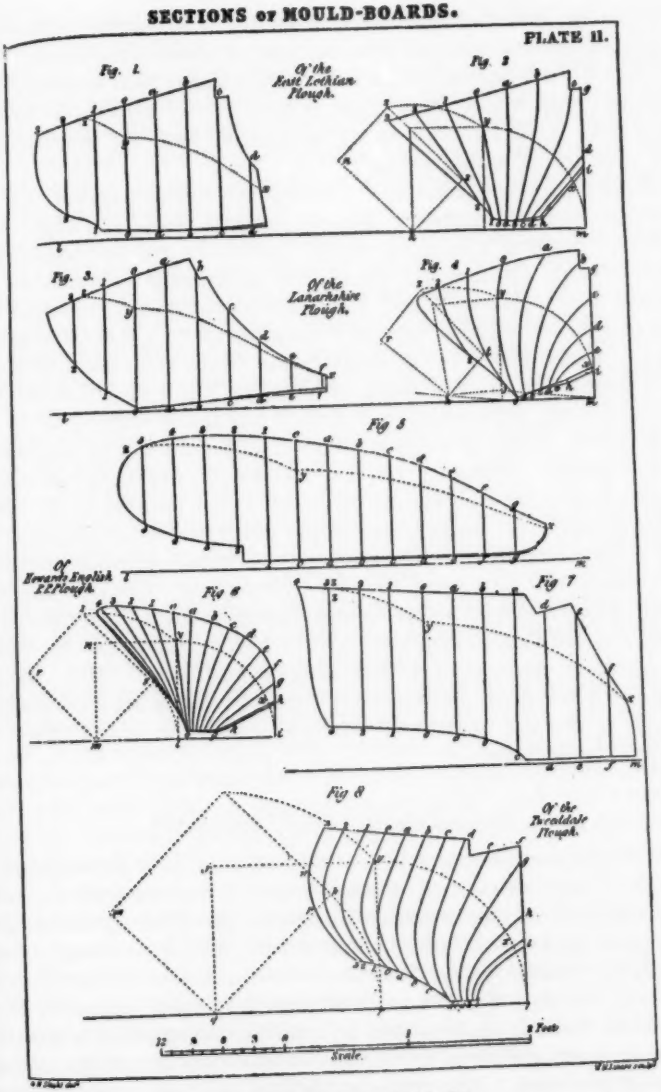
The lofty theorizing and abstract scientific inquiry fostered by Royal Societies of arts and science from the seventeenth century were seldom concerned with rude mechanical implements. Inventive effort was directed toward better drainage systems, improved plows, and seed drills, which, together with forage crops, turnips, crop rotation, commercial fertilizers, and improved breeds and seeds, would increase yields. The discovery of the advantages of deep plowing, which turned up the subsoil and allowed roots to penetrate deeper, and of seeding in drills spurred efforts to improve these implements.[8]

Field drainage, a major problem in parts of England, challenged engineering ingenuity in a pre-scientific era. Special drainage plows, including mole-type plows, were in use at an early date. Later, the Royal Agricultural Society offered prizes for tile-making machines, and Josiah Parkes (1793-1871), one of the first agricultural engineers, made elaborate calculations to ascertain the optimum diameter and depth of tile under various drainage conditions.[9]

Most scientific speculation, however, focused on the problem of designing a light draft plow. In response to a prize offered by the Society of Arts, a spring dynamometer was invented in 1783 to measure the motive force required.[10] Meanwhile, mathematical principles were employed to determine the ideal curvature of the moldboard (see Figure). Nevertheless, the practical use of mathematics proved negligible; the evidence points primarily to trial-and-error procedure.

For example, by 1730 a superior type plow, engineered in Holland, was being factory-produced at Rotherham (England).[11] Much later, after similar plows had appeared in America, Thomas Jefferson proffered his treatise on mathematics of moldboards; although hailed as a marvelous discovery, it was evidently of uncertain practical application. Neither the subsequent change from wooden to iron plows nor the development of the concept of replaceable parts utilized mathematics. John Deere's famous steel plow of 1837 featured a roughly diamond-shaped moldboard with a concave surface; and the shape continued to evolve over the next fifteen years.[12]

The famous treatise by Small (1874) illustrates the crudely scientific approach to plow design of this early period. The moldboard, he said, " must keep the plough in a proper hold on the ground; it must remove the earth to the furrow side, and it must turn it over." [13] Nevertheless, his own patent proved none too successful. The moldboard, for example, was too concave to work well in all soil types.



An early example of the use of mathematical principles to determine the optimum curvature of the moldboard of plows.

From *The Book of Farm Implements and Machines*, ed. Henry Stephens (Edinburgh, 1858).

Perhaps the difficulty was simply a matter of subjective opinion, for eventually several hundred different types of plows were manufactured in the United Kingdom to meet supposed peculiar local soil conditions. The fact that merely three types of plows today suffice for virtually all British farmers testifies to the unscientific basis for evaluating the performance of earlier plows.[14]

During the early nineteenth century the sciences applied to agriculture were soil chemistry, botany, genetics, and physiology; occasionally, physical principles, chiefly hydraulics and mechanics, were also utilized. Outstanding among the agricultural scientists of this period were Albrecht Thaer, a German physician who devoted himself to agricultural improvement; Humphrey Davy, a chemist employed by the British Board of Agriculture; and Justus von Liebig, professor of chemistry at the University of Giessen. These men laid the basis for agronomy, soil chemistry, and plant and animal nutrition.[15]

However, except in nutrition, no significant amount of engineering was required. Discovery of the nutritional advantages of making animal feeds easier to masticate and digest led to a profusion of complicated apparatus for cooking and steaming feed, and innumerable devices for cutting, slicing, mincing, crushing, and grinding fodder and feed. "Formerly our farm stock was fed with hay, or turned to pick over straw, sometimes with whole turnips thrown to them," commented the judges of the Great Exhibition in 1851 in their report on the variety of machines displayed for processing animal food.[16] This, then, is one of the few early illustrations of the interrelation between scientific discovery and the problem of practical application solved effectively through engineering.

Mechanization: The Empirical Era

Although one can point to a stream of prior inventions and slow, piecemeal improvement of existing devices, it was not until the 1830's that the pace of mechanization in agriculture definitely accelerated. The growing urban-industrial sector served agriculture in many ways, supplying manufacturing facilities, services, and a market for the increased output of farm commodities. Invention proceeded in an empirical fashion, characterized by undeniable inventive genius, but without much conscious application of scientific knowledge to engineering problems. The engineer, defined as "one who contrives, designs, or invents,"[17] was still associated with steam engines, undoubtedly because of the ingenuity required to construct and operate them. Efforts concentrated mainly on designing workable labor-

saving implements that would increase the output of labor and relieve back-breaking drudgery.

Since the impetus to mechanize was strongest in the New World, where land was abundant and labor scarce, technological progress in American agriculture rapidly eclipsed that of England.[18] While the Americans might have lagged behind Europeans in abstract scientific research and in the practice of scientific agriculture, they far excelled them in mechanical invention.[19]

One of the first "breakthroughs," the invention of a successful reaper, illustrates the empirical road traveled. McCormick's reaper, invented in 1831, was the culmination of a long line of precursors dating from 1786 (33 English reapers, 22 American, two French, and one German).[20] McCormick's invention was a fortunate combination of seven essential elements, namely, (1) cutter at one side and behind the horses, (2) heavy main traction wheel, (3) knife-teeth with serrated edges bolted on a horizontal cutter bar, (4) double fingers with a slot through which the knives vibrate, (5) a divider to separate the swath to be cut from the rest of the standing grain, (6) a reel to bend the grain toward the knife, and (7) a platform to carry the grain from the knife and lay it in a swath on the stubble side of the machine.[21] The finished machine was far from perfect. "To coordinate these parts," observes McCormick's biographer, "so that the machine would render its best service under almost any harvest conditions, required 15 more years of thought and experiment."[22]

An incredible number of patents were granted annually for mechanical inventions in the field of agriculture. In 1849, 117 patents were issued in agriculture, more than for any other group. Ten years later the annual total had risen to 659. Farm equipment manufacturers vied with each other in supplying the market with the "newest, improved" devices. Knowledge of improved machines and techniques was diffused by a growing number of farm journals and by exhibits and demonstrations at county and state agricultural fairs.

Rural fairs, which originated in Massachusetts in 1816, attracted the kind of practical farmer who had to be *shown* that new techniques were better than the old. Most emphasis was given to improved breeds and seeds and better farming practices, and although the first fair featured a plowing match, it was not until the 1840's that machinery exhibits, with contests between competitive implements, became a regular feature. By then agricultural fairs were a popular institution throughout the country.[23] Competitive trials were also a feature of world's fairs. At the Great Exhibition (1851) in London, McCormick's reaper astonished spectators by its flawless performance and won, not

only the award for its class, but a Grand Prize. In Paris (1867), McCormick's reaper and Wood's mowing machine won grand prizes and a special award from Emperor Napoleon III. Altogether, American implements received 39 awards at the Paris Exposition.[24]

The increased tempo of mechanization extended also to the smaller farm implements and tools. "Some of the most common instruments of husbandry have undergone a wonderful change in the course of a few years," reported the U. S. Commissioner of Patents in 1847. "The pitchfork, hand-rake and hoe, for instance, how different now from those which were common less than a quarter of a century since. The principles of science have been brought into requisition to mark out the relations and mechanical powers of the separate and their combinations; and much cumbrous, useless wood and iron has been removed, with gain, not only in lightness but in strength." [25] By the 1880's, over a period of fifty years, the curvature of axe and scythe handles achieved virtual perfection. Formerly these had been clumsily made, long and straight and of equal thickness throughout.

Reaping, threshing, and mowing by primitive methods are extremely time consuming.[26] The increased yields per acre (four or fivefold increase in European wheat and barley yields) and the larger acreages under cultivation (in America) after 1786 thus required mechanization of the harvesting process. When seed drills, improved plows, and better farming practices greatly increased both yield and acreage, the traditional methods of harvesting and threshing, with sickle, scythe, and flail, became hopelessly inadequate, although they lingered long on the scene. Their use in the United States continued until after the Civil War, while the comparative costs and the advantages or disadvantages of threshing grain by flail, horse, or steam were warmly debated.[27] Economic considerations were the major retarding factor, as was the case with machines which combined the harvesting and threshing operations. Grain combines were patented in the United States as early as 1828, but were scarcely used except in California wheat fields until after 1914; they were introduced into England in 1928 and into the Netherlands only after World War II.[28]

It is noteworthy that this initial technological revolution—the beginnings of mechanization—in agriculture began in a horse-drawn era, thereby severely limiting its engineering scope. When more sophisticated energy sources than draft animals or steam engines arrived, they unlocked the door to further mechanization. The internal combustion engine and electrical power, together with cheap steel and a well developed machine tool industry, widened the engineering horizon enormously, enabling more complex, versatile, or simply more

compact and mobile machines to be designed.[29] The present authors vividly recall that farm tractors were at first considered a mere substitute for horses. In fact, many farmers retained their usual complement of horses and operated under a dual power system. The 1920's marked the gradual transition to all-purpose tractor farming. The tractor, along with autos, trucks, surfaced roads, electric motors, telephones and radios, and a large assortment of new machinery adapted especially for work with tractors, spelled a whole new way of farming. "One might say that the explosions inside the cylinders of a gasoline engine were blowing apart an old way of life."[30]

Widening Scope of Agricultural Engineering

The possibility of applying scientific discoveries to agriculture, including implements, machines, and buildings of improved design, was widely acknowledged in agricultural literature after 1840. It was comprehended also that the initiative would have to come from individuals not concerned with immediate production problems.[31] Paradoxically, nineteenth century inventions were seldom the product of engineering activity, in which scientific knowledge is methodically applied to the solution of technical problems. The development of improved fencing is an excellent example of that fact. When the multitude of settlers pushed westward into the relatively treeless plains and prairies, the lack of suitable fencing material became a serious problem. Gathering data on types of fencing in use, the U. S. Department of Agriculture found (1871) an unbelievable variety, including some "the eccentricity of whose construction language very feebly conveys."[32] By 1881 no less than 1,229 fence patents had been registered with the U. S. Patent Office. Of these the barbed wire invented by Joseph F. Glidden, a farmer at DeKalb, Illinois, was to prove the answer to the settlers' prayers and to cause a revolutionary impact on the development of the American West.

The invention of barbed wire fencing clearly depended on a stage of technology in which low cost steel wire was available in quantity; indeed, the nineteenth century development in agricultural implements would have been inconceivable without prior improvements in the processes for making iron and steel. Even as the science of electronics today is opening up new horizons to the agricultural engineer, so the discovery of new steel-making processes made better designed, more durable, functional agricultural implements possible. Similarly, the development of gasoline motors and tractors depended upon scientific discoveries which harnessed electrical energy and released the enormous

potentials of petroleum fuel. Hence, technological advance in agriculture is closely allied to scientific discoveries and the development of technical processes in other fields. Furthermore, since the over-all process of discovery and invention is cumulative and chain-reacting, each successful development paves the way for new inventions in formerly unknown terrain or along paths previously overlooked or deemed too difficult to traverse. In recent years, for example, science has reinforced technology in solving the problem of mechanizing the harvesting of fragile crops by creating new varities of plants better suited to machine operations.[33] This would seem to be the only solution for mechanizing such tasks as the harvesting of grapes and tomatoes. Already the development of dwarf varieties of sorghum, hardier cotton plants, shatterproof grain, and special varieties of corn and sugar beets have substantially reduced the damage and waste resulting from machine methods of harvest. The devising of automatic machinery for performing routine chore labor involved in poultry and livestock production represents a similar challenge to the engineer.

Typically, in agricultural technology, the basic concept was of early origin. The first crude specimens failed to meet practical tests or were uneconomic, and were discarded, only to be improved later. Also, typically, a considerable time elapsed before even a successful invention was widely used. The faltering course of technical progress in agriculture may be seen in the grain drill which evolved over a period of 39 centuries.[34]

A more intimate relationship between science and engineering is seen in the history of food technology, notably dairy products. The need for an accurate milk-testing device had long been felt, if for no better reason than to forestall the proverbial temptation of milkmen to sell watered milk. The Commissioner of Patents, in 1860, reasoned that since " many persons can judge pretty accurately as to the quality of milk by carefully observing the transparency of the fluid when poured in a thin film from one vessel to another," it should be possible to develop some kind of measuring instrument.[35] This conjecture foreshadowed twentieth century spectrophotometric and electronic methods of color sorting, sizing, and testing fruits, vegetables, eggs, and seeds.[36]

Actually, the first significant engineering development in milk-testing came in 1890 when Stephen M. Babcock, professor of agricultural chemistry at the University of Wisconsin and chief chemist for the Agricultural Experiment Station, devised the standard butterfat test. In a public report Babcock explained that he deliberately sought a method that would be simple, foolproof, and involve no expensive

apparatus or chemicals. He then invited manufacturers of diary supplies to develop the unpatented method on a commercial scale.[37]

The scientifically engineered butterfat test was the climax of a long series of developments in dairy products technology in which science worked hand in hand with engineering. Agricultural literature after 1800 is replete with discussions concerning improved designs for dairy barns to ensure adequate ventilation, lighting, and sanitation. In 1853, after ten years of experimentation, Gail Borden devised a successful method for evaporating milk in a vacuum, and sterilized condensed milk in tin cans was used by armies during the Civil War.[38] The silo, of French origin, came into wider use in the 1870's, about the same time that thermometers began to be used in making butter and cheese. Refrigeration was developed about 1875; and in 1877 the Swedish engineer and scientist, Gustav de Laval, invented an apparatus which, by centrifugal motion, separated cream from milk. Laval's cream-separator was introduced into the United States about 1882.[39]

These remarkable innovations came in response to the accelerated urban-industrial growth and the attendant expansion of the dairy industry. But it was only after a much larger fund of scientific knowledge had accumulated, after the Industrial Revolution had entered an advanced stage, and after new and versatile sources of power and structural materials became available that significant progress in the mechanization of all branches of agriculture became possible. This eventuality also implies the existence of a body of men well trained in scientific subjects and capable of the systematic and persevering effort that solving basic engineering problems demands.

Emergence of Agricultural Engineering as a Professional Field

Agricultural engineering, the application of physical science to the solution of problems in mechanized agriculture, has existed as a profession for only a half century. In 1907 J. B. Davidson, an instructor in farm mechanics at Iowa State College, Ames, Iowa, called together a number of his colleagues at other institutions to organize a society of farm mechanics teachers.[40] Following considerable discussion, the 40 or 50 persons attending, including representatives from the U. S. Department of Agriculture, farm journals, and the fledgling farm equipment industry, elected to form a society of professional engineering calibre called The American Society of Agricultural Engineers. This organization, through its technical sessions and publications, became the focal point for presentation and publication of technical data pertinent to the field. Within its structure four rather broad fields

of specialization have evolved: Power and Machinery, Farm Structures, Electric Power and Farmstead Processing, and Soil and Water Conservation. Today, with almost 6,000 professional and affiliate members, membership in the ASAE is practically a *sine qua non* for development and advancement as an agricultural engineer in the United States.

Although not so well documented, the development of similar organizations in other countries apparently parallels that of the United States. Western Europe has become active in agricultural engineering through the efforts of numerous organizations. According to Hopfen, some 500 agricultural institutions, governmental and non-governmental, now exist in 88 countries of the world.[41] They publish 54 journals, some of which are international in scope.[42]

Shortly after the ASAE was founded, departments of agricultural engineering began to be established in various Land Grant Colleges in the United States. The first such department, at Iowa State College, in 1909, was headed by Davidson. Today, 44 such departments conduct research and teaching in the United States, of which 33 are approved by the Engineers' Council for Professional Development to grant the B. S. degree in agricultural engineering. About 40 additional institutions throughout the world now grant four-year degrees in agricultural engineering. Meanwhile, the horizons for engineering activity in agriculture are continually being pushed outward by advances made in scientific fields, and the long-standing evolutionary progress based on cumulative knowledge and invention proceeds at an ever-quickening pace.

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Science and the Steam Engine

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THERE IS A commonly held notion that the development of the steam engine took place with little support from science and a sort of corollary to this that the steam engine did more for science than science did for the steam engine. Lewis Mumford has put it this way:

> The detailed history of the steam engine, the railroad, the textile mill, the iron ship, could be written without more than a passing reference to the scientific work of the period. For these devices were made possible largely by the method of empirical practice, by trial and selection. . . . And although all these inventions would have been the better for science, they came into existence, for the most part without its direct aid. It was the practical men in the mines, the factories, the machine shops and the clockmakers' shops and the locksmith's shops or the curious amateurs with a turn for manipulating materials and imagining new processes, who made them possible.[1]

J. D. Bernal says, "The successive developments of the steam engine—separate condenser, expansive working, compound cylinder—right down to the steam turbine at the end of the century were essentially successive essays in engineering, solved in practice before they were solved in theory." [2]

Professor Ubbelohde takes a somewhat less sweeping position: "However, really scientific principles concerning heat and mechanical power were not applied to the steam engine before the work of James Watt." [3]

This idea that the steam engine was, somehow, a craft invention and its corollary, that the scientists were uninterested in such practical devices, persists and recurs in various contexts. Of course there are many factors that combine to produce a complex technology such as that based on steam power—economic, social, political, geographical, etc.—in addition to the scientific and technological ones, but it is the

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latter that we are concerned with here. At the very least, the above quotations on the relations of the steam engine and science exaggerate their independence even for the early history of the steam engine. It is our feeling that there has been a strong interaction between science and steam engine technology, and in this paper we will call attention, very briefly, to some aspects of this interaction.

The history of the steam engine is reasonably well documented and the reader is referred to the usual sources for the detailed narrative and drawings of the various machines.[4] Its invention was mothered by the necessity of draining the mines and providing water for the growing cities, needs which had become pressing by the beginning of the seventeenth century. Giambattista della Porta (1606) was the first to demonstrate, on a laboratory scale, that steam could be used to move water, either by forcing it or by drawing it into a vacuum formed by condensing steam. After this, numerous attempts were made throughout the century to raise water by so utilizing steam. At least those attempts which are recorded were made by men of means and of learning rather than "the practical men in the mines," etc.

In 1641, Galileo was consulted by the engineers of Cosmo de Medici II who wished to build a vacuum pump that would draw to a height of fifty feet. Although Galileo failed to solve the problem, the investigations of his pupil Torricelli and the subsequent work of Pascal, von Guericke, Hooke, and Boyle led to the establishment of pneumatics as an exact, mathematical science and to the development of practical equipment for pumping both gases and liquids. Von Guericke's work was especially significant for the later development of the steam engine because he showed how work might be obtained by the movement of a piston in an evacuated cylinder. Huyghens in 1678 and his assistant Denis Papin experimented with an engine in which a piston was moved by the explosion of gunpowder. Upon the revocation of the Edict of Nantes, Papin, an outstanding man of science, fled to England where he became acquainted with Robert Boyle. He then worked in Italy for a time and later settled in Germany where he occupied the Chair of Mathematics at Marburg.

By 1690, Papin had abandoned gunpowder for steam and proposed to obtain motive power by the force of atmospheric pressure upon a piston behind which was a vacuum produced by the condensation of steam. He wrote: "Since it is a property of water that a small quantity of it turned into vapour by heat has an elastic force like that of air, but upon cold supervening is again resolved in water, so that no trace of the said elastic force remains, I concluded that machines could be constructed wherein water, by the help of no very intense

heat, and at little cost, could produce that perfect vacuum which could by no means be obtained by gunpowder." Papin actually devised a laboratory apparatus which, while suitable for illustrating the validity of his concept, was not a practical machine.

And so the stage was set for the great act of invention by Thomas Newcomen, using the principles first arrived at by Papin. Professor Usher tells us that "the invention of the atmospheric engine in this form was the greatest single act of *synthesis* in the history of the steam engine and must be regarded as one of the primary or strategic inventions. The important work of Watt and his contemporaries was critical rather than synthetic, and though new devices had to be invented, they are, after all, improvements of the Newcomen machine, which can scarcely be rated as greater inventive achievements." [5]

We have italicized Professor Usher's use of the word synthesis in order to emphasize that there were at least two lines of development that culminated in Newcomen's work. On the one hand there was the line from the air pump to the piston-in-cylinder principle in the hands of von Guericke, Hooke, Boyle, Huyghens, and Papin. But there was also unceasing effort throughout the seventeenth century to build a practical engine utilizing della Porta's concepts directly; here we need only refer to the high points associated with the names of Salomon de Caus (1576-1626), Edward Somerset (1601-1667), and Thomas Savery (1650-1715).

It was to Thomas Savery, F. R. S., military engineer, scientific experimenter, civil servant, and the most prolific inventor of his day, that a patent was granted (1698) which preempted the field for steam engine invention. Steam was used first to create the vacuum and then to obtain the pressure successively to draw and push the water in the manner suggested by della Porta. A small number of such engines may have actually been set up in mines and to provide country houses with water. Leibniz visited England in 1705. Upon seeing a machine of Savery, he described it to Papin who, in 1707, turned aside from the cylinder and piston engine to make a similar one. But Savery's engine finally proved to be an unworkable failure, not because of a failure to employ scientific principles properly, but rather due to imperfect workmanship and materials. Rhys Jenkins has pointed out that "today there would be no difficulty in making a successful working machine to the drawings given. . . ; indeed in a modified form, and with an automatic steam valve, Savery's principle is embodied in the modern pulsometer." [6]

We are not certain of the extent of Newcomen's debt to Savery and Papin. An ironmonger, he may have actually been employed in

the erection of the early Savery engines; after Savery obtained his patent, Newcomen joined forces with him making his own engines under that patent. Newcomen's achievement was to transform the concept, first conceived by Papin, into a practical machine—a heavy, substantial device of brass, iron, and wood that pumped the mines of water so efficiently that it made money both for the mine owners and the company of profit-seeking businessmen that acquired Savery's patent.

If one considers the limited range of materials available for construction and the utter crudeness of the machine tools of the time, the mere building of a machine that would work successfully under practical operating conditions must be regarded as a feat. But Newcomen was not a mere worker in wood and metals. He was a man of learning, versed in science, and he maintained an active correspondence with Robert Hooke. Contemporary records indicate that Hooke probably kept him informed of Papin's progress, including the principle of the condensation of steam under a piston. Certainly by the time Papin published his plan in 1690, Newcomen was already concerned with the improvement of Savery's engine and would have been receptive to such a notion. Scholars are not agreed on the validity of any direct connection between Papin and Newcomen through the intermediacy of Hooke; Dickinson is doubtful, while Usher and Jenkins tend to believe in such a connection.

In any case, the picture of steam engine technology that emerges is certainly more subtle than mere empiricism operating in the dark, far from the light of science. It would certainly appear that, at this stage of development, the science of pneumatics was sufficiently advanced and the steam engine engineers were well enough versed in that science so that it could provide real guidance to the emerging technology. Of course any engine must be a practical device, and any inventor a practical man. No matter how firmly established upon scientific principles an engine might be, the inventor still has problems: there is the matter of the fitting of the parts and the strength of the materials and the necessary devices that permit it not only to operate but to operate efficiently, which means economically. The seventeenth-century scientists were vitally interested in the practical utility of science, and many of their problems were oriented towards subjects useful for technical development.[7] But one hardly expects, even for cases of the strongest interaction between science and technology, that a completed process or a finished machine is to be the product of the scientist's laboratory. In this light, it would certainly seem that in the case of the development leading to the Newcomen

engine we have a striking illustration of the interdependence of science and technology.

With the commercial success of the Newcomen engine from about 1712, further advances until the time of Watt were primarily structural and mechanical. Smeaton made tremendous practical improvements by systematic measurement and study rather than the introduction of new principles. These improvements in the Newcomen engine could be brought about only as the tools and processes for accurately finishing the parts that had to fit together smoothly were improved and as materials such as iron both increased in strength and became more readily available. This did not involve direct participation of scientists and scientifically oriented engineers nor did it even require recourse to scientific knowledge other than that already available to Papin, Savery, and Newcomen. It was a matter of craftsmanship catching up. It may be such *improvements* that Mumford has in mind in the paragraph quoted at the beginning of this paper. Of course, as R. J. Forbes has pointed out,[8] the properties and strengths of materials were first studied both experimentally and theoretically by scientists such as Galileo, Hooke, Mariotte, LaHire, Rondelet, 's Gravezande, Coulomb, Euler, Réaumur, etc., and it is upon the foundations laid by them that the machine industry itself was based.

By improvements in design and execution of the parts, the Newcomen engine attained the ultimate in performance. Beyond this point, further advance was dependent upon technical innovation, and this was consummated by James Watt. His invention of the separate condenser led to a great advancement of steam engine technology. The case for the dependence of technology upon science is frequently made to rest upon Watt's presumed indebtedness to Joseph Black for his discovery of the latent heat of steam. The matter is certainly more subtle than this, and Watt himself reacted rather sharply to the implication of such an oversimplification:

> . . . though I have always felt and acknowledged my obligations to him for the information I had received from his conversation, and particularly for the knowledge of the doctrine of Latent Heat, I never did, nor *could*, consider my improvements as originating in those communications. . . .
>
> But this theory, though useful in determining the quantity of injection necessary where the quantity of water evaporated by the boiler, and used by the cylinder, was known—did not lead to the improvements I afterwards made in the engine. These improvements proceeded upon the old-established fact, that steam was condensed by the contact of cold bodies, and the later known

> one, that water boiled in *vacuo* at heats below 100°, and consequently that a vacuum could not be obtained unless the cylinder and its contents were cooled every stroke below that heat.
>
> These, and the degree of knowledge I possessed of the elasticities of steam at various heats, were the principle things it was necessary for me to consider in contriving that new engine.[9]

But if Watt's invention did not emanate directly from a single scientific discovery of Black's, neither can it be isolated from the scientific environment in which he was immersed. As "mathematical instrument maker to the University of Glasgow," Watt's introduction to the steam engine came when he was asked by Professor John Anderson, during the session of 1763-64, to repair a model Newcomen engine belonging to the Natural Philosophy class and used for instructional purposes. His only previous experience with steam had been with a Papin digester with which he had experimented a year earlier.

It is noteworthy that Watt was introduced to the steam engine as a laboratory instrument rather than as a machine. And his approach was dominated by this. He analyzed the engine scientifically. He studied the physical processes involved and, where necessary, carried out independent experiments such as those which led him to the rediscovery of the latent heat of steam, to the relation between pressure and boiling temperature, and to the pressure-volume relations for steam. With these discoveries alone he would have made his mark as a natural philosopher of unusual talent.

Watt had available as friends and consultants two of the best scientific minds of the age, Black and John Robison, who not only encouraged him but lent him money to support his experiments. Indeed, very early in his career he had collaborated with Black on another problem of applied science—a process for the manufacture of alkali by the decomposition of lime with sea salt.

Watt was not only immersed in a scientific atmosphere, he was isolated from practical technology. He had no experience with the construction of full size engines. Yet possibly it was to his advantage to experiment with laboratory scale models. He could proceed with a degree of imagination and with a freedom from preconceived notions not possible for the practical engineers, weighed down by six decades of traditional engineering practice. The practical part of Watt's career came only after he was well launched upon the inventive part, when it became necessary to construct and to promote a commercial engine.

Watt maintained an active interest in science throughout his life.

A member of the Lunar Society and a Fellow of the Royal Society, he numbered among his friends and acquaintances the leading scientists of the age, European as well as British.

As Professor Usher points out, there is no consistency in the interrelation between invention and the various environmental conditions, such as science, geography, economics, etc., to which one might like to relate the emergence of the invention. Only "an empirical analysis of the events themselves" can lead to the proper insights in each particular case. Novelty in mechanical invention emerges at many levels. At the lowest level, invention is a consequence of new acts of manipulative skill, and at the highest it is based upon orderly scientific knowledge.

In the invention of the steam engine we are concerned with a cumulative process, taking place over a period of two centuries and involving thousands of steps in which the inventiveness occurs at many levels. How, then, shall we classify the level of this invention? Certainly not by averaging the achievement of the boy operator who suggested an automatic valving system for the Newcomen engine (apocryphal?) with that of Newcomen or Watt. The level must be set by the high water mark, and so the conclusion is inescapable, almost obvious, that the steam engine is a product of the age of science and is derived from science.

After Watt, steam came of age and powered the industrial revolution. Its full exploitation was brought about by the utilization of the expansive working of steam and the development of the high pressure engine. Although work on a steam turbine was sufficiently advanced to cause Watt some anxiety, this device did not prove successful until the late nineteenth century. Watt himself had discovered and employed the expansive working of steam, and if he was cool to the high pressure engine, others both in his own organization and outside of it quite clearly understood its advantage. Thus, at the turn of the nineteenth century there was once again a plethora of new concepts, and, just as a century earlier, progress again depended upon engineering practice rather than scientific innovation. The fabrication of boilers and the precise machining of parts became the problem.

This does not mean that science was completely out of the picture during the nineteenth century. Much of the steam engine and turbine design, especially in France, was carried out by the new class of professional engineers. It is true that England had still not faced up to the problem of providing industry with scientifically trained engineers, and the new high pressure engines were built by men like Richard Trevithick who had no formal education. But even Trevithick

did not operate in a scientific vacuum but frequently consulted with his lifelong friend and mentor Davies Giddy, who became President of the Royal Society. "What would be the loss of power in working an engine by the force of steam raised to the pressure of several atmospheres but instead of condensing to let the steam escape?" asked Trevithick. Giddy replied: ". . . one atmosphere diminished by the saving of an air pump with its friction and in many cases with the saving of condensing water." Not exactly a conversation of the workshop.

In France the great engineering schools, l'École Polytechnique, l'École des Mines, l'École des Ponts et Chaussées, and many others in the provinces, supplied the army, the civil service, and industry with engineers steeped in the best tradition of science and research. For the first time, the modern pattern of putting scientific and engineering training in the hands of the *leaders of research* was practiced. High standards of scientific education were set, and in France during the first half of the nineteenth century the quantity of good scientific and engineering work surpassed that of England.

The French engineers hardly considered themselves other than scientists; the most successful of them were elected to the section on mechanics of the Academy of Sciences and occupied chairs at the engineering schools or the University. Frequently the Academicians were called upon to evaluate new inventions or to supervise the awarding of various prizes for inventive achievements. The pages of journals such as the *Comptes Rendus*, *Journal de l'École Polytechnique*, or the *Bulletin de la Société d'Encouragement de l'Industrie Nationale* were available for the publication of technical memoirs, and the level of technical textbook writing was especially high.

It is significant that the next major *breakthrough* in steam technology is associated with the name of Charles Parsons for his invention (1884) of a successful steam turbine. This followed nearly a century of progress primarily with water turbines in which scientifically trained French engineers played the leading role. Parsons himself had the benefit of the best possible scientific education. His father, Lord Rosse, was an astronomer and President of the Royal Scienty. Tutored at his palatial home, which was a center for visiting scientists and where his father's telescope was located, he was immersed in an environment that led him to study mathematics at Cambridge. The steam turbine was designed to drive the new electric generators, and the problems were so complex that they could only be resolved by combining all the available resources of mathematics, science, and machine design, a task far beyond the most brilliant mechanic.

Finally, we might consider the young French steam engine engineer, Sadi Carnot, who, starting with a consideration of the steam engine, arrived at a principle which was ultimately developed into one of the most fundamental pronouncements in all of science. The history of the acceptance of Carnot's work [10] and the implications of this major scientific advance arising in such a manner [11] have each been dealt with at some length elsewhere. In a memoir [12] which can only be described as a work of genius, Carnot developed a theory of the steam engine and heat engines in general. As extended and formalized by Clausius and Kelvin,[13] this led to the science of thermodynamics with its well known ramifications both for science and technology.

It is with Carnot's contribution in mind that the remark to which we have referred in our opening paragraph—about the steam engine doing more for science than science did for the steam engine—is most frequently made. There is an implication here that, since Carnot's theory of the steam engine came *after* its primary development, this somehow buttresses the premise that the steam engine developed with little reference to the body of science. The development of the engine would have been enhanced had the theory been available, and since the engine did actually develop in the direction indicated by the theory, this development, so the argument appears to run, must have been empirical and non-scientific.

If this were actually the case, there would hardly be a technological advance that we could say is rooted in science. Science provides for technology various instruments, data, concepts, theories, etc. It never provides the ultimate in theory. Actually, it need not even provide the latest in theory in order to make the claim that a particular technological innovation was nurtured by science.

Consider for example the matter of modern nuclear technology. It could hardly be claimed that *this* has developed without roots in contemporary science. And yet there is no theory of the atomic nucleus. The empirical facts themselves, as unravelled in the laboratories, forced the concept of fission upon the scientific consciousness, and the bomb and reactors were built without a theoretical understanding of the fission process. Should a later day Carnot develop such a theory from which can be extracted confirmation of present practices or prescriptions for improving the nuclear fuels, would it be meaningful to say that nuclear technology had done more for science than science for technology? And even if one were to make this value judgment, does it follow, then, that the development of nuclear technology has been empirical and non-scientific?

It seems to me that this is the way matters stand with the steam

engine. The sciences of pneumatics and mechanics were sufficiently developed to provide the conceptual basis and the practical guidance needed to develop a steam engine technology. In fact, we have seen that it was engineering practice that lagged behind. The machine tools and the materials needed to exploit fully the designs of Savery, Newcomen, and Watt were not available. The development of high pressure engines had to await improvements in boiler construction. The designers were conversant with and took advantage of the available body of scientific knowledge.

The contributions of Carnot and Watt to science serve to emphasize the dynamic nature of the relation between science and technology. Carnot's contribution led to further scientific developments, which in turn provided the basis for whole new technologies. The connections between science and steam engine technology were neither simple nor were they static. But one thing is certain. Steam engine technology cannot be considered apart from the body of science.

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Science and Engineering in the History of Aviation

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I

AN ATTEMPT to evaluate the roles of science and engineering in the history of aviation [1] must of necessity begin by making it clear what is meant by "science" and "engineering." Clarity requires that a definition be simple rather than complex. Consequently, at the risk of oversimplification, we will define science as the accumulation of knowledge about the physical world, both through the gathering and classification of factual data and the formulation of basic laws and principles, and engineering as the application of this knowledge to purposes deemed useful to man.[2] In other words, the scientist wants to know chiefly for the sake of knowing; the engineer wants to know for the sake of using.

This is a distinction more likely to be blurred than not. Scientific research has often been undertaken in order to elicit information which is needed for a specific purpose, or with the idea that the results will have an eventual applicability. It is in fact a fair guess that more scientific knowledge has come from this source than from strictly "pure" research. On their side, engineers have frequently taken on the functions of the scientist because they have had to fill gaps in the theoretical background or the body of data available for what they wanted to do. Nevertheless, the distinction has to be made if we are to evaluate accurately the importance of contributions to science and technology. The two are related, but neither synonymous nor identical. There have been long periods of history when science and engineering developed pretty much independently of each other; this indeed was the usual situation until just before the Industrial Revolution. Conditions have changed considerably since, but it is still possible for a historian to defend the validity of these two propositions: one, that for technological advance it is not necessary that the whole body

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of underlying scientific theory and information be known; two, that even if the theory and data are complete (an ideal state not yet encountered in experience), it does not follow that they can forthwith be put to practical use.

The history of aviation offers a number of useful tests of these propositions. Man dreamed of flight for many centuries before he could transform the dream into reality, so that there is a long record of speculation on the subject and occasional experimentation, none of it of any real value in solving the problem until late in the eighteenth century. Leonardo da Vinci, it is true, had some shrewd insights, but his ideas were lost for too long to influence the pioneers of aviation, and his own efforts were hopelessly blocked by the inadequate technology of his time. To compound his difficulties, his concepts of flying machines ran to the helicopter, whose problems were not solved until the 1930's, and the ornithopter, or flapping-wing device, which is still unsolved.[3]

The effective beginning of aviation dates from 1783, when successful balloon ascensions were achieved in France.[4] The Mongolfier brothers, who were first in the field, used heated air in their balloons: in fact, they believed at first that the lifting effect came from the smoke and they did not really know why their idea worked. There was, however, an obvious disadvantage to carrying an open fire into the air, and Professor J. A. C. Charles, a physicist, found a more promising gas in hydrogen, isolated by Henry Cavendish in 1766.

The balloon naturally suggested the airship, but the gap between concept and execution was a broad one. Except for a moderately successful experiment by a French engineer, Henri Giffard, with a steam-powered dirigible in 1852,[5] the airship had to wait until the twentieth century, not only for the internal combustion engine but for the patient efforts of men like Alberto Santos-Dumont and Count Zeppelin in working out the problems of structural design and control mechanisms.

Systematic and useful study of heavier-than-air flight began at about the same time as the balloon experiments. As early as 1810 Sir George Cayley, who has as good a claim as anyone to be considered the first real aeronautical scientist, put his finger on the fundamental problem: namely, "to make a surface support a given weight by the application of power to the resistance of air."[6] He rejected the ornithopter idea for the fixed wing and even suggested some sort of internal combustion engine as the probable power plant. His followers made enough progress so that in 1876 a twenty-seven year old Frenchman, Alphonse Pénaud, was able to publish a patent for a flying machine

with double surfaced planes set at a slight dihedral angle, a large aspect ratio, and cambered wings.[7] In short, by 1876 the theory of powered flight was quite well understood; the stumbling block was the engineering problem of designing an engine which would be light enough to get off the ground and at the same time powerful enough to drive the machine through the air. There were some experiments with steam. John Stringfellow succeeded in flying steam-propelled models in England in the 1840's, and Samuel P. Langley did the same thing in the United States fifty years later,[8] but the results did not encourage further development in this direction.

Power, of course, was not the only problem. Striking as had been the nineteenth century advances in the study of the problems of flight, there was still need for testing of theories and for the accumulation of a body of data by actual flying experience with some man-carrying device rather than with models. This was the contribution of the great gliding pioneers like Lilienthal, Pilcher, Montgomery, and Chanute. They tried, by rule of thumb added to the small body of existing knowledge, various wing and body designs and control mechanisms in actual flight. They compiled some useful information about the behavior of flying machines, and they made the inevitable mistakes, fatally in the case of Lilienthal and Pilcher, which constituted invaluable guideposts for their successors.

When the twentieth century arrived the maximum essentials for powered flight had come into existence: some fundamentals of aerodynamics, a body of experimental data, and the internal combustion engine as a power plant with a potentially low enough weight-power ratio. So we come to Samuel P. Langley and the Wright brothers.

Langley was preeminently a scientist—self-taught astronomer and physicist, with a brief experience at the beginning of his career as a practicing civil engineer. The story of his experiments in flight from 1887 to 1903 is one of endless patience and determination in the face of constant disappointment from unforeseeable mishaps and inferior workmanship. He almost succeeded because his assistant, Charles M. Manly, a mechanical engineer, designed and built for him a five-cylinder radial engine developing 52 horsepower and weighing 125 pounds.[9] It would be pointless now to revive the bitter controversy between the partisans of Langley and the Wrights. We do not know, for instance, if Langley had solved the problem of lateral stability, which he like the Wrights regarded as more difficult than the matter of propulsion, although we do know that Glenn Curtiss rebuilt and flew Langley's machine in 1914. The best indication is that Langley's "aerodrome" would have flown in 1903 if it had not fouled its

lauching gear on both occasions when Manly tried to get it into the air.

On the other hand, suppose Langley had spent less time in calculation and in experimenting with models and more in actual gliding flights as the Wrights did. Might he not then have realized that he did not really need his elaborate catapult contrivance, perched on top of a houseboat in the Potomac? It is an engineering truism that the more complicated a mechanism is, the greater the likelihood of something going wrong.

The Wright brothers are difficult to classify as either engineers or scientists, because they were educated as neither but functioned on various occasions as both. Their story is too well known to require repetition here. For our purpose what is required is to point out that their basic approach was that of the engineer: accumulating all the available information, designing and testing their own models and gliders, like Manly building their own engine because no manufacturer was willing to undertake to construct so light a motor.[10] They became scientists of necessity because information they needed was either inaccurate or non-existent. In 1901, Wilbur Wright concluded that existing tables of figures on lift surfaces were in error, and the brothers then compiled their own by constructing a wind tunnel and testing some 200 wing surfaces in it.[11] Then, when they came to the consideration of power, the brothers discovered to their astonishment that although propellers had been used in ships for over half a century, no body of propeller theory existed, and they had to work it out mathematically for themselves.

In other words, it would be a grave error to write off the Kitty Hawk achievement as a triumph of cut-and-try by a couple of tinkerers. The Wrights prepared themselves not only by flying gliders but also by mastering the existing body of aeronautical science and adding to it on their own.

II

At this point, we have got man into the air on the strength of a little science, somewhat more engineering, and a long hard process of trial and error. Since it is clearly out of the question to deal in a single paper with all that has happened since, I wish to select those features in the subsequent history of aviation which bear most directly on the influence of and relationship between science and engineering.

First, conspicuously, is the striking growth of aeronautical science after the possibility of flight had been demonstrated. The determination of the aeronautical pioneers from Cayley to the Wrights becomes

all the more impressive when we realize that they had to contend not only with the derision of the uninformed but with an impressive body of scientific theory indicating that they were actually attempting the impossible. Yet no less a figure than Sir Isaac Newton had made calculations of air flow on wings showing much less lift than was actually obtained, while the existing body of theory in fluid mechanics gave no evidence of lift or drag in perfect fluid flow.[12] Not until the work of Kutta in Germany, Jukowski in Russia, and Lanchester in England between 1902 and 1907, was the existence of lift firmly established in aerodynamic theory.

Once the break was made, research in aeronautics expanded rapidly, for some time considerably more so in Europe than in the United States. American opinion rather astonishingly ignored the achievement of the Wright brothers and watched apathetically while Europe forged ahead in aviation. There were some exceptions. Glenn Curtiss, introduced in 1907 to heavier-than-air flight through Alexander Graham Bell's Aerial Experiment Association, unquestionably led the field in the development of flying boats, an engineering rather than a research operation,[13] and in 1913 M. I. T. installed an aerodynamics laboratory with a four-foot wind tunnel.[14] Large-scale aeronautical research in the United States got its effective start when the shadow of the First World War stimulated Congress to create the National Advisory Committee on Aeronautics in 1915, with instructions to "supervise and direct the scientific study of the problems of flight, with a view to their practical solution."[15] In executing these instructions, the NACA covered the entire spectrum of aeronautical science and engineering: it engaged in basic research, and it studied specific technical problems submitted to it by the government and the aviation industry.

Much of this research has provided the basis for practical developments in the design and construction of aircraft. The advance of aerodynamics, for example, has made possible improvements in airfoil and airframe design which have approximately doubled lift per square foot, and reduced drag by a factor of three-fourths since the first decade of this century.[16] For the layman whose memory goes back far enough, aeronautical science here represents the difference between the sleek-lined monoplane of the present and the elaborate structure of struts and wires which characterized the airplanes of forty years ago.

This story can be repeated throughout the whole field of aeronautics. Research in problems of stability and control, stresses, the chemical composition of fuels, and so forth, has been an indispensable component of progress in aviation. It would be easy, indeed, to conclude

that the course of development has fallen into a definite pattern of establishment of basic principles and data followed by specific application; in other words, that after the cut-and-try pioneering days ended, aeronautical engineering became dependent on aeronautical science.

But did it? With due respect to advances in other areas, the outstanding factor in improving aircraft performance has been the spectacular development of the power plant, and this has been predominantly an engineering achievement. To illustrate from American experience, the 12 h. p. engine used at Kitty Hawk grew into the 12-cylinder, 400 h. p. Liberty of the First World War with no significant change in design. The radial engine which came into use during the 1920's was a new departure in practice but not in principle; the basic idea was incorporated in the engine Manly built for Langley in 1903. By the end of the Second World War, propeller-type engines had attained 3000 h. p. by a steady process of refinement.[17] The engine was capable of further improvement, but by this time speeds and altitudes had reached the effective limit of the propeller.

The next step was the jet, easily the major breakthrough in propulsion in the history of aviation. In popular terminology it is no doubt considered one of the miracles of modern science. The basic principle, however, has been recognized for centuries. Hero of Alexandria used a jet of steam in his "aelopile" in the first century A. D.[18] Admittedly, this gadget was nothing but an interesting toy; the rocket, however, is a form of jet propulsion, and rockets have been in use for military and other purposes since the thirteenth century.[19] How many Americans realize when they sing of the "rockets' red glare" that it was something more than a poet's imagination or a display of fireworks put on by the British to entertain Francis Scott Key?

The basic law of jet propulsion is simply Newton's third law of motion, that every action has an equal and opposite reaction. Here we have an illustration of both the propositions advanced at the beginning of this essay. Rockets were in practical use for 400 years before anyone explained why they worked; and the fundamental principle underlying jet propulsion was understood for another 250 years before it became possible to master the technical details required to convert Newton's law into a functioning jet engine.

The creation of a jet engine which would out-perform the piston engine was an engineering problem, or rather a series of engineering problems. The idea which opened the way for a solution, the use of a gas turbine to provide a high-pressure jet, originated in the mind of a twenty-two year old Royal Air Force cadet in 1929. The principle

of the gas turbine was not new either; the idea can be traced back to 1791.[20] However, when young Frank (now Sir Frank) Whittle conceived of applying the gas turbine to jet propulsion, he still had a dozen years of frustrating effort ahead of him before he had an engine operating in actual flight.[21] His difficulties were not matters of fundamental principle, but technical points like the proper setting of turbine blades or the control of air turbulence in the compressor.

III

The last phase of this story can be appropriately introduced by quoting from an entry in the weekly reports of the Engineering Department of Convair for April, 1945.

"Work has just started on possible airplanes for supersonic speed. . . . A turbo-rocket power plant seems to be the most promising at the present time. Practically no aerodynamic data are available in this speed range, and what there is isn't consistent." [22]

In other words, the high-pressure development of jet propulsion which the Second World War stimulated had put into our hands a power plant which provided both new opportunities and new problems for aeronautical science. Aerodynamic properties, for example, change radically at sonic velocities so that supersonic speeds and very high altitude flying involved much more than putting a jet engine into a conventional airframe—or even an unconventional one. The swept-back wing, for instance, was found to be well suited for high-speed flight, but some adaptations had to be worked out to allow for the fact that an aircraft still has to take off and land at low speeds.

Let me quote again, this time from J. H. Kindelberger of North American Aviation: [23]

> In air-frame design, which had been refined nearly to the ultimate of low-drag for the 400 m. p. h. region, new trends were started involving radical changes in shape, control systems, equipment, and structures. But each solution brought new problems as near-sonic and transonic phenomena were explored in wind-tunnel and flight trials.
>
> For example, the instability experienced with swept wings at near stalling speeds resulted in experiments with various solutions. . . . As is always the case, the problems seem relatively simple when the solutions have been found, but in the frantic period of exploration it often appears that the laws of physics are capriciously defying our best efforts.

It may be, indeed, that aviation, especially as it moves into the realm

of missiles and space flight, has become so radically changed as to invalidate generalizations which were applicable up to about 1950. Certainly the aeronautical engineer of today has to be much more of a scientist than his predecessor of fifty years ago. The history of aviation, nevertheless, shows a constant pattern of what John L. Atwood, President of North American Aviation, defines as "nibbling at the edge of the unknown."[24] At some times aeronautical science has opened the way for further engineering achievements; at others technological advance has made it possible to go beyond the existing limits of scientific knowledge.

Is there accordingly a meaningful distinction between science and engineering in aviation? For the historian there is, if history is to be an accurate evaluation and interpretation of past experience. An intelligent differentiation will clarify the significant steps in the development of aviation, and it ought to illuminate the fact that aeronautical science and aeronautical engineering have been complementary rather than competitive. Man is not quite in the position of the bumblebee, which we have been told is constructed in violation of the laws of aerodynamics but goes ahead and flies anyway. Man has the privilege of thinking about what he is doing. Still, we would never have got off the ground if some of our number had not been willing to try something without having to know the answers in advance.

REFERENCES

[1] The research on which this paper is based has been supported by a grant from the American Philosophical Society. This paper was read to the session on the History of Technology at the annual meeting of the Pacific Coast Branch of the American Historical Association, San Jose, California, August 30, 1961.

[2] For a much more complete and satisfactory definition, see Eric A. Walker, "Engineers and/or Scientists," *Journal of Engineering Education*, vol. 51, no. 5 (Feb. 1961), pp. 419-421.

[3] F. A. Magoun and Eric Hodgins, *A History of Aircraft* (New York, 1931), p. 8.

[4] For a good contemporary account of the early balloon experiments see H. Garnerin, *Aeronautica* (London, 1802).

[5] Magoun and Hodgins, *op. cit.*, p. 123.

[6] *Ibid.*, p. 251.

[7] E. C. Vivian and W. L. Marsh, *A History of Aeronautics* (New York, 1921), pp. 16-17.

[8] *Langley Memoir on Mechanical Flight* (Washington, D.C., 1911), pp. 3-5. The partisans of steam have not been as vocal in aviation as in automobile history, but they exist. We can grant that if enough effort and ingenuity had been

expended on it, the steam engine might have been adapted to the needs of aviation. Once the lightweight internal combustion engine became available, however, no one thought it worthwhile to continue with steam.

[9] *Ibid.*, p. 250. The gross weight of the power plant, including fly wheels, battery, gas and water tanks, was 187½ lbs.

[10] Fred C. Kelly, *The Wright Brothers* (New York, 1943), p. 87.

[11] Wilbur Wright "Some Aeronautical Experiments," Address to the Western Society of Engineers, Sept. 18, 1901, in *Papers of Orville and Wilbur Wright*, M. W. McFarland, ed., vol. I (New York, 1953), pp. 99-118.

[12] For this background of aerodynamic theory, I am indebted to Drs. Warren E. Wilson and Jack Alford of the Department of Engineering at Harvey Mudd College.

[13] *Aircraft Yearbook*, 1921, p. 140, Curtiss, a manufacturer of motorcycles, first entered aviation by making engines for small airships.

[14] "The Airplane Problem," *Aviation*, vol. IV (1918), p. 778.

[15] George W. Gray, *Frontiers of Flight* (New York, 1948), p. 13.

[16] John K. Northrop, *Aviation History*, address given in Library of Congress, Nov. 3, 1948, p. 8.

[17] *Ibid.*, pp. 20-21.

[18] R. S. Kirby, *et al.*, *Engineering in History* (New York, 1956), p. 54.

[19] Willy Ley, *Rockets, Missiles, and Space Travel* (New York, 1951), p. 51.

[20] Sir Frank Whittle, *Jet* (London, 1953), p. 30.

[21] The Germans were the first to fly jet aircraft. Whittle believes that they used his basic ideas, which had appeared in technical journals before the war, and that they sacrificed quality in order to rush production. See *Ibid.*, pp. 289-290.

[22] Ms. History of Convair, A Division of General Dynamics Corporation, p. x-1.

[23] J. H. Kindelberger, "Design of Military Aircraft," *Aeronautical Engineering Review*, vol. 12, no. 12 (Dec., 1953), p. 6.

[24] John L. Atwood, *Some Aspects and Problems of the Aircraft Industry*, address at Eighth Stanford Business Conference, Stanford University, July 28, 1949, p. 7.

Book Reviews

The Construction of Gothic Cathedrals: A Study of Medieval Vault Erection. By John Fitchen. Oxford: Clarendon Press, 1961. Pp. xxi, 344. 73 Illustrations 63 s.

The medieval churches, cathedrals, and castles of Europe have for generations attracted the attention of art and architectural historians and have called forth innumerable studies of the aesthetic and structural aspects of these buildings. More recently, and especially in England, researchers have recovered the biographies of many medieval "architects" and have revealed the economic aspects of medieval masonry construction. In spite of the misleading titles of several well-known books, no one had written a book solely devoted to the technical processes of medieval building construction until Professor Fitchen published the present study. Surely his book will call forth further studies on this long-neglected subject.

Professor Fitchen states his purpose and method of presentation by noting:

> . . . this present study is an attempt to combine, in effect, two books in one. The first is represented by the illustrated text proper, designed substantially as an entity by itself. It is directed to that segment of the general public, including teachers and their pupils, who wish to get some basic understanding of the probable medieval techniques and practices of vault erection, including their relationship to the dynamic structural innovations and developments that evolved during the Middle Ages. The second intention has been that of providing the scholar with fairly thorough exploration, analysis, and documentation (via the Notes and Comments, the Appendixes, and the Bibliography) of the material found in the text. (p. xiv)

Professor Fitchen is basically concerned with the rediscovery and description of the temporary timber-work that was used in erecting the permanent stone vaults of medieval cathedrals, and more specifically (as he himself chooses to define his subject), of those cathedrals constructed by the "French Gothic builders." He begins with an analysis of the sources of information, and one by one he rejects the medieval chronicles, manuscript miniatures, extant working drawings, and building accounts as incapable of revealing any really pertinent information on his particular problem. Professor Fitchen has a point here, as anyone who has worked in these materials very well knows, but he has been a bit stringent in rejecting them *in toto*. It leaves him with no information on the methods of construction except the facts that can be obtained from the extant buildings. This much he admits: "And so we return to the buildings themselves for information and enlightenment on the problem of construction" (p. 7).

Since the information concerning the problem is neither abundant nor unambiguous, Professor Fitchen is obliged to use deductive arguments based on *a priori* assumptions of the methods of medieval builders. He does this with consummate skill, as he deduces his way through four chapters of closely reasoned arguments. He methodically analyzes and rejects previous theories (especially those of Viollet-le-Duc) in slowly building up his own version of the type of centering used in building the vaults. Apart from the conclusions tentatively reached, the most important contribution of these chapters, and perhaps of the whole book, is the clear statement of the particular problems involved in constructing vault centerings with the materials and equipment used by the medieval carpenters. Numerous excellent line drawings by the author himself of vaults and centerings play not only an essential role in the development of the arguments, but also considerably aid the reader's understanding of those arguments through "pictures worth ten thousand words." Then as the reader begins to compliment himself on having successfully followed the maze of argumentation to its expected conclusion, Professor Fitchen in the last chapter lightly rejects his own hard-won results in favor of a hypothesis for the building of the vault webs without the use of centering. The hypothesis was first suggested over a century ago by M. de Lassaux, architect to the King of Prussia, though it has received scant attention since then. This neat trick of vault construction was accomplished, it is suggested, by ropes which were secured at one end and weighted with rocks at the other, and which were hung over each stone of the web as the web courses were laid between the ribs of the vault. The backward and downward forces that resulted from the secured and weighted ropes held the stones in place until the succeeding course was placed on top of them. This process was repeated for each course to the top of the vault.

Professor Fitchen accepts this as the method used by the French Gothic builders, but we wonder if perhaps he has not leaped to a brilliant solution that possibly could have worked, but that may not have been used. He has provided a theory of the possible method used by the French masons in constructing vaults: this theory remains now to be tested by historical investigation into the various sources that might confirm or deny its authenticity. Considering the European-wide influence of the French masons on both the structural and the constructional development of medieval building, one would expect that somewhere evidence more positive than Professor Fitchen has offered might be found of the actual use of this method. This may yet come to pass. Meanwhile one cannot help being puzzled by the fact that Filippo Brunelleschi created a furor in Florence in 1417-18 when he proposed to build the brick panels of the dome of *Il Duomo* with a type of centering that required no timber under-support. (See

F. D. Prager, "Brunelleschi's Inventions and the 'Renewal of Roman Masonry Work,'" *Osiris*, IX (1950), pp. 457-554, which Professor Fitchen has omitted from his extensive bibliography.) Why should Brunelleschi's "invention" have been so revolutionary if the French masons had been building vault webs without any centering for two centuries? Could the "Italian builders" have been completely ignorant of this important constructional practice of their northern confreres? It hardly seems likely. The investigation of problems such as this will in time test the validity of the "Lassaux-Fitchen theory" and will provide a more precise delineation of the times and places wherein this method was used, if at all.

Professor Fitchen deserves the warmest praise for his beautifully executed study. If he has not provided answers that will satisfy everyone, his book is nonetheless significant, for he has rendered that other inestimable service to scholarship: he has raised questions and uncovered problems for further investigation.

L. R. SHELBY *

American Building Art: The Nineteenth Century. By Carl W. Condit. New York: Oxford University Press, Inc., 1960. Pp. xvii, 371. Illustrations. $12.50.

The author of this book is widely known for his many contributions to the history of building technology. An earlier book by Dr. Condit, *The Rise of the Skyscraper*, is an indispensable aid to the understanding of the Chicago school of architecture. This present volume deserves the attention not only of specialists, but of general readers as well. Dr. Condit has a prose style which is far from pedantic; its vigor matches the drama and adventure which is always associated with major construction projects.

Dr. Condit discusses the following topics: wood framing, iron framing, wooden bridge trusses, iron bridge trusses, suspension bridges, iron arch bridges, railway trainsheds, and concrete construction. The period he has chosen, the nineteenth century, marks the beginning of a transition from empiricism to an ever increasing reliance on physical and mathematical theory in the development of American structural design, a transition which is still far from complete.

We are given an insight into the origins and historical antecedents, in ancient and medieval times, of the various structural systems under consideration, and evaluations of their aesthetics and their influence on

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architecture. The great personalities of nineteenth century American engineering and architecture are also given their due. Eads, Jenney, Roebling, Adler, and Sullivan are the men whose great achievements are featured in this work.

The section on the railway trainshed is particularly interesting. The problem faced here of enclosing a huge space with a minimum of supports is similar to that posed by the great aircraft hangers, warehouses, and exhibition halls of the twentieth century.

The book is richly illustrated, and well bound and printed. The concluding notes should not be neglected, as they contain a wealth of valuable and interesting information.

Many avenues worthy of further exploration are apparent to any reader of this book. That is as it should be with any work which is among the first in its field.

HARRY MAHLER *

France and the Economic Development of Europe 1800-1914. By Rondo E. Cameron. Princeton, New Jersey: Princeton University Press, 1961. Pp. xviii, 586. $10.

Throughout the nineteenth century France was second only to Great Britain as a capital-exporting country. Until Napoleon III's *coup d'état* in 1851 most French capital went abroad either to purchase government bonds or to accompany French entrepreneurs and technicians who invested it directly. Throughout the Second Empire and until 1882, when the depression beginning in that year checked the outflow, new capital went to purchase the securities of foreign corporations which Frenchmen usually had been directly or indirectly instrumental in launching. Thereafter, and until the outbreak of the First World War, Frenchmen diverted most of their new private investments to areas outside Europe; that part which continued to flow to European countries ordinarily took the form of loans to those governments which the Third Republic thought were politically important. Professor Cameron appropriately entitles the chapter dealing with these government loans "Capital Down the Drain: the Government Loan Business." In all three periods, but especially during the second, the typical French investor bore little resemblance to the conservative *rentier* stereotype found in most textbooks. On the contrary, he was an enterprising lender of venture capital who often chose to invest abroad rather than at home because of the possibility of obtaining a higher rate of return.

Frenchmen not only exported large amounts of capital, but they also

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diffused technology and entrepreneurship. Napoleon I and his armies carried abroad many political, legal, and social institutions which were essential to modern capitalism; French technicians, engineers, scientists, and skilled workers spread industrial and financial know-how by serving abroad and by training foreigners who visited France. Even though her own economy was already beginning to decline compared with the economy of certain other nations, France was able "to contribute so importantly to the economic development of Europe" in the nineteenth century "by intellectual, social and legal influences, by the diffusion of technology, and by the export of capital; in combination, these means generated yet another determinant of economic growth, the spirit of enterprise" (pp. 502, 503). The diffusion of technology and the stimulus provided for capital accumulation, unsurprisingly, proved most successful in those countries where a rational legal system, agrarian reforms, general literacy, and their own political and social institutions properly conditioned the people.

Parts I and IV ("Determinants of Economic Progress" and "Epilogue and Conclusions") make for fascinating and facile reading. Professor Cameron judiciously summarizes the basic trends in French and European economic development and expertly uses the comparative method to expose the most responsible factors involved. Not only is his literary style polished and sparkling, but Professor Cameron also intersperses his analysis with astute observations and snatches of wisdom which are all too rare in studies similar to his. He recognizes, for example, that material improvement and human progress are not necessarily the same thing. His two concluding sentences are worth repeating: "The experience [of France in European economic development] serves to illustrate the fact that capital and even technology are neutral instruments in themselves. Like the dynamite or atomic enegry which they can purchase or create, they may be used to promote human welfare or destroy the human race" (p. 510).

Parts II and III ("Sinews of Growth: Credit and Transport" and "Case Histories of Development and Disappointment") constitute the hard core of his study. It is here that he presents the facts and relationships he uncovered during more than two years of research in private and public archives scattered over Western Europe. This does not make for quick, easy reading, for the factual information is too rich and the intercorporate relationships too bewildering. Yet how could it be otherwise if the author attempts to demonstrate how French financial institutions (especially the Crédit Mobilier) influenced economic development at home and abroad and served as the pattern for financial institutions in other countries? How else can he faithfully trace the Frenchman's role in the organization and construction of railways on the continent? A master map for Europe, supplemented by a number of regional ones, vividly shows which railroads were built or owned by companies predominantly French, which ones were built

with the assistance of French capital or by French contractors, and which ones resulted from little or no French participation. Professor Cameron also recounts the amazing career of the Pereire brothers and tells of their rivalry with the Rothschilds in French and European finance, railroads, and industry. None of these is a simple task; to accomplish all of them in a few hundred pages represents a tour de force of which any economic historian would be justifiably proud.

WARREN C. SCOVILLE *

The Union Pacific Railroad: A Case in Premature Enterprise. By Robert William Fogel. (The Johns Hopkins University Studies in Historical and Political Science, Series LXXVIII, Number 2.) Baltimore: Johns Hopkins Press, 1960. Pp. 129. $3.50.

This is not "another" history of the Union Pacific (this is one of the book's many merits). Rather, Professor Fogel has intentionally omitted or treated cursorily the debates on alternative routes, the scandals in Crédit Mobilier, and the physical progress of construction, for he recognizes that these are well-known. More to the point, he deals with questions of public economic policy as posed by this premature enterprise and focuses his attention on his thesis "that the building of the road was pushed ahead although it had not yet matured as a profitable private enterprise, made government intervention inevitable and thus gave rise . . . to a series of complicated issues on the relationship of government and business."

At a time when mixed enterprise in internal improvements was not extraordinarily novel, the Union Pacific represented a persistent dichotomy between sound private investment principles and public economic necessity, with the latter rather than the former providing the force that energized this project. Fogel's careful analysis of the Congressional debates on bills prior to the Acts of 1862 and 1864 leads him to conclude that the very practical issue of how to finance this eminently desirable (and certainly difficult) railroad scheme was foremost. Out of this came a general recognition that the government necessarily and inevitably had to intervene, so that debate centered first on the degree and second on the manner. At no time down to 1860 was there a headlong flight from a government road, but there were concerted efforts at avoiding that kind of participation. Yet, in

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each proposal offered, there was a continuing thread of private enterprise and public responsibility in a variety of combinations. Put pragmatically, many Congressmen welcomed propositions for a mixed enterprise in this project for they believed work would never begin unless such assistance was offered.

Out of this evolved the legislation of 1862 and 1864, which was neither a plan for a government work nor an unrestricted invitation to private investors—in other words, it was the partnership between government and private capital preferred by Congress. Yet within a decade, the Wilson and Poland committees found "ample evidence" of fraud, bribery, and personal profiteering to condemn the UP's chief officers and their counterparts in Crédit Mobilier. On this point, Fogel holds that the promoters did not cause the company's financial instability but rather places this responsibility with Congress. He suggests that the men who wrote the enabling legislation did not recognize the kinds of problems involved in undertaking a premature enterprise. Believing the amount of capital needed to build the line was the central issue, they provided the familiar land grants and bonds as inducements to construction. But the risk attached to the capital put up by the promoters was nearer the heart of the dilemma, and this apparently was not appreciated by Congress. Earlier businessmen had not built such a railroad solely because the anticipated profit was not large enough to justify the required investment. Then, as if to worsen the problem unintentionally, Congress increased the risk to private capital by imposing a huge bonded indebtedness on the Company. Professor Fogel supports this general view by tracing the promoters' difficulties down to late 1867 in selling their bonds; because the public doubted the line's earning capacity, the bonds were not readily taken but necessarily sold at lower prices. By carefully calculating the component elements of the Company's receipts and expenditures, Fogel has determined that the actual profit fell somewhere between 13 and 16½ million dollars, a figure considerably lower than that settled on by the Wilson committee.

Certainly, as Professor Fogel recognizes, an evaluation of the UP's financial condition must include a consideration of the social rate of return on the capital invested. To the Congressional proponents of this line, the growth of the nation's economy was of such strategic importance that the Company might necessarily earn less than the market rate of return, and it was partially on this ground that Congress intervened. The author, by ascertaining the value of land on a strip forty miles on each side of the right-of-way from Omaha to Ogden before and after construction, contends that the national income was increased by some 15½ million dollars in 1880 due to the productivity of labor and capital on these lands. From this, then, "there can be little doubt that the government was economically justified in intervening to build a road that would not have been built by unaided private

enterprise." Thus, the irony of the enabling legislation and actual experience is clear—for both private interest and public policy to be best served, the line should have been built by the government (thereby shifting the risk to itself) and then sold to private investors about 1875 at its capitalized value. When done, the government would have netted a fine profit, the UP would have been a sound business enterprise, and the scandals probably would never have occurred.

This little volume takes a well-known event and subjects it to a skillful and penetrating analysis. Drawing on formal economic theory (interest theory, theory of rent, and the concept of present value) to determine and scrutinize relevant historical facts and using a newly-contrived mathematical formula to arrive at a "justifiable profit," Fogel has presented historians with a provocative and compelling interpretation. This might well serve as a model for studies of similar enterprises.

ROBERT E. CARLSON *

Vanishing Crafts and Their Craftsmen. By Rollin G. Steinmetz and Charles S. Rice. New Brunswick: Rutgers University Press, 1959. Pp. 160. Illustrations. $4.75.

One consequence of the Indusrtial Revolution was the slow disappearance of the hand craftsmen who had for centuries been the backbone of manufacturing. Yet a few such craftsmen managed to survive into the twentieth century, practicing their crafts much as their ancestors of the eighteenth and nineteenth centuries. The authors of this book have written brief chapters on a series of individuals who still practice crafts rapidly vanishing from the American scene. Included in the survey are a blacksmith, wood carver, lime burner, candy-maker, charcoal burner, cigar makers, a maker of wreaths and baskets, a man who produces wooden scoops and bowls with an ax, a potter, an illuminator, a family which long produced horn buttons and combs, glassmakers, an ox-yoke maker, and, somewhat curiously, a one-room school.

This brief volume has no scholarly pretensions. Instead it seems to have been written to cater to a resurgent popular interest in the handicrafts. Most of the examples are taken from Pennsylvania and New York. The prose is journalistic, sometimes folksy, and often nostalgic. There are some brief, non-technical descriptions of processes used by the craftsmen. The chief attractions of the volume are the numerous and excellent photographs of the craftsmen at work. The scholar will

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learn nothing new from this volume, but for the general reader it may well provide a pleasant and nostalgic couple of hours.

KENDALL BIRR *

Black Rock: Mining Folklore of the Pennsylvania Dutch. By George Korson. Baltimore: The Johns Hopkins Press, 1960. Pp. xi, 453. $7.50.

This book is arranged in twenty chapters, of which the first nine are devoted to a rather uneven chronological background of the anthracite industry among the Pennsylvania Dutch. With Chapter 10, the author takes up in considerable detail the various aspects of life in the region, principally the customs, behavior, and folklore. The chronology is never completed and there is a considerable "paste and scissors" character to the last eleven chapters. The index is good. The footnotes, which are listed as "source notes," are carried following the last chapter, and there is no bibliography nor a discussion of sources or methods of collection. The book would have benefited considerably had it been illustrated, and there are no maps, which the nature of this book demanded. Many sources of importance to the story of anthracite in the region are omitted. There is very little discussion of the changes in mining technology which would have increased the importance of the work.

This reviewer has the impression that the book will be of interest principally to those interested in the Pennsylvania Dutch or who have come from that fascinating region. Much of the folklore has been treated to one extent or another in some of the author's earlier works, which occupy an important place in the history of American mining—*Songs and Ballads of the Anthracite Miner; Minstrels of the Mine Patch; Black Land; Coal Dust on the Fiddle;* and *Pennsylvania Songs and Legends.*

The author has cleared considerably the story of Philip Ginder, the "folk hero" of the book and the man generally credited with discovering anthracite. Ginder's importance was that his "discovery" occurred at the right time, and it was followed by events which ultimately brought into existence one of the most important industries in American history and from which has been derived more wealth, from just one great bed, than any other mineral deposit in the world. One of the unfortunate omissions from this book is any discussion of the effects of this industry—beyond mere customs or folklore—upon the social and economic environment of the people of eastern Penn-

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sylvania. For the economic and social history of the peoples of the anthracite regions, we must refer to out-of-date government reports or scattered studies by sociologists. The only adequate source for the anthracite industry as a whole is the now unavailable book published in 1932 by the Hudson Coal Company, supplemented by the very valuable contribution of Carl Corlsen (pseud. of A. W. Fey, *Buried Black Treasure*, 1954).

CHARLES O. HOUSTON, JR. *

Steelworkers in America: The Nonunion Era. By David Brody. Cambridge, Mass.: Harvard University Press, 1960. Pp. 303. $5.

Using material which has been well combed over before, principally testimony given in connection with various government investigations, David Brody in his *Steelworkers in America* has taken a fresh approach in defining the labor-management relationships in the iron and steel industry during the period from 1890 to 1929. He has focused his attention on the steelworkers and studied the process by which their working lives were shaped in what he terms the "nonunion era" of the industry.

In developing his thesis, Mr. Brody has set as his aim the examination of "such operative elements as life in the mill towns, the immigrant background of the unskilled, the role of the 'English-speaking' group, the recurrence of unemployment, the technology of steelmaking and the aims of management," and the effect each had on the laboring groups. Mr. Brody presents convincing arguments to prove his point that it was a combination of all of these factors which served to establish a period of labor stability during the nonunion era which only the steel strike of 1919 was able to disturb.

His book is comparatively brief, but he gives a clear and interesting account of the changing situations which were brought about by the effects of increased mechanization on skilled and unskilled labor alike. He has understood the adjustments which had been necessary in the changeover from the production of iron to, first, Bessemer and, at a later period, open hearth steel. The role which the influx of large numbers of non-English speaking immigrants from Eastern Europe played in the labor picture is well told.

In the early days of the Bessemer steel industry, competition certainly existed, but not until the coming of such financial leaders as Andrew Carnegie did it become ruthless. At first, the equipment which the various companies had installed and their capacities to produce steel were remarkably similar. Therefore, the difference in economy rested

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primarily on labor costs. The industry worked around the clock and the twelve-hour shift was the order of the day. For a time, Captain Jones had tried the eight-hour shift but he had not been able to maintain it. The iron and steel managements required some time to face the issues of shorter hours, improved living conditions, as well as better pay for the unskilled labor (the skilled workers had always been well provided for). If the wages were too low, they were still considerably higher than those paid in Europe, and, for that reason, thousands of immigrants were attracted to this country. It must be remembered that the early part of this century was the era of the sweatshop, and the iron and steel industry did not run counter to the labor practices of the day. The rights of the working man had yet to be universally recognized.

The period after 1910 Mr. Brody calls the "cooperative era," and considers at some length the company welfare policies which served to stabilize the laboring force. To its credit, the steel industry was the leader in promoting safety measures, no easy task when the majority of the laborers were unable to understand the language or prone to think of such precautions as "sissy." Several of the companies, principally the United States Steel Corporation under the aegis of Judge Gary, made efforts to improve the appalling living and working conditions of the immigrants. During the period 1912-1920, the Corporation spent some $70,000,000 for such purposes as welfare, sanitation, accident prevention, relief for families of men injured or killed, and Americanization programs for the foreign-born.

Eventually, under the pressure of public opinion and with the development of a more enlightened attitude on the part of management, many of the worst conditions were either considerably improved or eliminated entirely. What Mr. Brody has written, favorable or otherwise, is all part of the record.

There is little that can be questioned in what he says. One might consider his opening remarks, however, in which he states that the British steelmen visiting the United States in 1901 marveled at the progress of the iron and steel industry and were unsettled by the mechanical perfection of American operations. He goes on to say that thirty years earlier the mills had been backward and inefficient and that the annual pig production had not reached one-sixth of the English output.

Thirty years earlier would have been the year 1871. While the Edgar Thomson Works had yet to be built, the American Bessemer steel industry, barely five years old, was already making its way. Only ten years later, in 1881, the English journal *The Engineer* could publish, "The United States ironmasters are beating us 100 per cent in the output of their plant. With one pair of converters they can do as much and more than we can do with two pairs; and while our blast furnaces turn out 480 tons of pig per week, theirs, much smaller, give as much

as 1,100 tons a week." The article then goes on to note the better organization and the better equipment which had made such production figures possible. As a matter of fact, the year before this had been written, the United States had succeeded in out-stripping England in Bessemer steel production.

In his comparison of American and British pig iron production, Mr. Brody perhaps underestimates the early and phenomenal growth in the output of pig iron by the American producers. Thus, in 1871, instead of producing less than a sixth of the English output as Mr. Brody states, the United States was more likely turning out over a fourth. The next year the figure had increased to more than a third, and in 1890 the American mills exceeded the British production by more than a million tons and were accounting for 34.1 per cent of the world's production. One might also add that in the same year the Americans turned out 35 per cent of the world's steel production. Thus, it would seem that the British had reason to feel unsettled considerably earlier than 1901.

Mr. Brody has contributed a provocative and interesting study of one of the most dramatic periods in American industrial history and one which can well serve as an authoritative text for some time to come.

JEANNE MCHUGH *

Blue Collar Man: Patterns of Dual Allegiance in Industry. By Theodore V. Purcell, Cambridge, Mass.: Harvard University Press, 1960. Pp. xii, 300. Illustrations. $6.00.

This book is a study of the worker's dual allegiance to his union and company. The employer is Swift and Company at its plants located at Chicago, Kansas City, and East St. Louis. Three unions represent the worker at these plants: The United Packinghouse Workers, National Brotherhood of Packinghouse Workers' Union, and The Amalgamated Meat Cutters Union. Union leaders at the Chicago plant are mostly Negro; the other two plants are white-dominated. Theodore V. Purcell, S. J., the author, is a Roman Catholic priest and is Associate Professor of Psychology at Loyola University.

This study is mainly the results and conclusions drawn from about eight hundred interviews with workers, foremen, and union leaders. Using scales of ratings, these interviews are summarized in statistical tables. These data are also supplemented by union statistics and other

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published reports. From 1949 to 1951, the author lived near the Chicago plant; in the summer of 1953, he lived not far from the Kansas City plant; and in the fall of 1953 and winter of 1954, he was in Goose Hill in the heart of East St. Louis. The basic purpose of the author was to find out the extent to which the worker in the meatpacking industry had a dual allegiance to his union and to his company.

Better to understand the worker and the place he works, the author begins with a study of the plants and their neighborhood environments. In all, about 8,000 employees work in the three plants. There are no important differences in the age of the plants or working conditions. The proportion of Negro workers is higher in Chicago, and there is also in all the plants a considerable number of Mexican workers.

In contrast to the plants, the unions differ widely. In Chicago, the union is said to be unstable, anti-management, and torn by internal dissension. In the two other plants, the local union is more secure, conservative, and pro-management. The serious struggle over the Communist issue in the United Packinghouse Workers is widely known. Over the past ten years there have also been bitter conflicts between the unions themselves.

The role of the worker as an employee is first considered. "Putting everything together," what does the worker think in general about the company? Other important aspects of the job include the worker's attitude about his pay, the company suggestion box, the "gang" itself, working conditions, and the worker's aspirations for his children. Due to differences in management and union leadership, the employees were much better satisfied with the company in East St. Louis than in Chicago or Kansas City. Working conditions are substantially the same in all of these plants. Due to their southern background, Negro workers in East St. Louis show much less dissatisfaction than those in Chicago. Despite all the differences due to location, racial composition, sex, etc., there was a clear existence of a favorable allegiance to the Swift Company.

After the study of the attitude to the company, the author then takes up the employee's relation to his union. As a whole, what does the worker think of the union and its leaders? Is he satisfied with its system for handling grievance? To what extent does the worker actually participate in union activity? Although much weaker in Chicago, it was found that there was a strong and continued allegiance to the union in all of the groups. Only in Chicago did the hourly-paid worker register a neutral attitude toward the union and its leaders. Allegiance, for example, was extremely high in the East St. Louis plant. The attitude of the union leaders did not differ much, regardless of their union. Finally, as a test of these stated attitudes, the author shows how the union member has reacted under strike conditions. The worker believes in the necessity of a union and will support it in time of crisis. As a rule, he does not favor a strike.

Allegiance may follow three patterns—union, company, or dual. Contrary to popular opinion, the extent of dual allegiance is unbelievably large. A number of problem areas exist which should be improved, such as a lack of pride in the work done, dissatisfaction with union leadership and policies, and the lack of any active participation in union affairs. The author finds in this dual allegiance the possible roots of industrial peace. A deeper loyalty to both company and union would promote a better regard for the work done and union participation.

As the author shows, there is a rather large number of studies in this particular area of industrial labor relations. Ross Stagner and others studied dual allegiance in *Illini City*, a University of Illinois study. Another similar work was by Seidman and his associates in *The Worker Views His Union* (University of Chicago, 1958). The author cites at least a dozen or more other studies dealing with the same basic problem. Although these studies differ in approach and methodology, they support the main findings of this book.

Although perhaps outside the scope of this study, there is considerable material which clearly points to the serious problems growing out of automation and plant modernization, for there has been a movement to do away with the old and larger plants and to change to a smaller, one-story, mechanized plant.

Automation has increased greatly in the past ten years. Pictures are given of one sausage-stuffer which replaced five women workers, and also of some of the new electric machines for cutting and hide removal. Several of the workers interviewed thought that the suggestion system might bring devices which would displace workers, and they felt that this would not be a moral act.

In his discussion of the 1959 strike, Father Purcell deals with the Armour automation fund, a plan by Armour to lessen the burden of unemployment caused by automation. To be financed by the Armour Company up to $500,000, the plan attracted national attention and was well received by the unions.

Serious problems of displacement due to automation do exist in the meatpacking industry. Many workers will not only have the question of dual allegiance, as in the case of a strike, but will be faced with the necessity of finding entirely new jobs. A number of experts have been asked to assist in lessening the impact on employment of these technological changes. When Armour shut down its Oklahoma City plant in the middle of 1960, Professor Richard Wilcok and his colleague, Professor Walter Franke, of the University of Illinois Institute of Labor and Industrial Relations were asked to work on the problem of dislocated workers. *Business Week* has summarized some of the important facts about the operation of the Armour Plan and automation in meatpacking in two recent articles (July 16, 1960 and April 15, 1961). It was estimated that over the past five years employment by

the Armour Company had declined from 60,000 to 40,000. At the same time, with automation, it had been able to produce more meat. *Business Week* also quoted figures from the United Packing Worker's Union to the effect that 30,000 jobs had been lost in meatpacking during the last four years despite some increase in production. It was also predicted that possibly one out of four workers in the industry's total number of workers might be displaced.

One concludes this book with the feeling that it is one which will be read by both union leaders and management. It gives a valuable insight into the feelings and attitudes of men who work for one of our great mass production industries. There is ample evidence that Father Purcell in his role as priest-psychologist has been able to secure the complete confidence of his friends, the blue collar workers. Real people speak and they speak freely. The general reader will be greatly surprised at the extent of this dual allegiance.

The general methodology of the book is one which has come into wide use by social scientists in the last few decades, but one about which sociologists and others do not always agree. Father Purcell has by unusual insight and painstaking effort been able, by using a scale of favorability, to translate the information from his interviews into quantitative data which are meaningful. Some of the interviews are now about ten years old, and about one half are seven and eight years old. While this allows for a spread over varying economic conditions, the data may not fully reflect changes in attitudes due to such factors as racial conflicts and automation.

The author hopes by a better understanding of this area of industrial relations to bring about an increased loyalty to both the union and the company. Whatever may happen in this regard, the meatpacking worker will be confronted with immediate and serious problems on at least two issues–the problem of race relations and the problem of human displacement by plant modernization and automation. No one can doubt the growing dissatisfaction of Negroes and other small minority groups with their underprivileged status. At this time there are many who are beginning to think of the problem of technological unemployment as beyond the power of any single company, and one that must be dealt with on a national level.

LAWRENCE R. CHENAULT *

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Armor: A History of Mechanized Forces. By Richard M. Ogorkiewicz. New York: Frederick A. Praeger, 1960. Pp. x, 475. Illustrations. $7.95.

Although this book was written primarily with a military audience in view, it contains a wealth of insight that commends it to those interested in the history of technology. In a field characterized by highly specialized monographs, usually national in scope, the author has attempted an encyclopedic survey. After a brief introductory discussion of theory or doctrine emphasizing the historic interrelationship of fire-power, mobility, and armor, a series of chapters describe the widely different conceptions regarding the proper use of armored forces evolved by the leading military powers between 1915 and the present. Against this essential backdrop of organization and theories of tactical employment, the author recounts in nine separate chapters how the tank has been developed in Britain, France, the United States, Grmany, the Soviet Union, Italy, Japan, Poland, and Sweden. Tanks hold the center of the stage, but the other elements of mechanized force, self-propelled guns, armored personnel carriers, anti-aircraft tanks, etc., are given some attention. For the student of technology probably the most interesting chapters are the five comprising a group on the problems of design, the development of components, including armament and power plants, etc., as well as some brief remarks on production and costs.

Richard M. Ogorkiewicz, a lecturer at the Imperial College of Science in London, has been a close student of armored warfare for many years. He is widely known in military circles within the United States for his frequent contributions to the professional journal, *Armor*. Without question the author has made many solid contributions to the study of mechanized warfare; unfortunately his achievement in this book is marred by numerous defects. The volume reads as if the author had pulled together a series of articles or lectures without careful editing; its chapters overlap; undefined technical terms crop up repeatedly, and, more significantly, it is undocumented save for a bibliography of some seventy-odd published sources. Moreover, the author is an unabashed partisan. Even on the most obviously moot points of doctrine he writes with dogmatic assurance of "the proper" solutions while stigmatizing "erroneous theories" with the utmost confidence. In short, the author presents polemical essays rather than objective history; even where the reader is inclined to agree with the views offered, the tone employed is so offensively doctrinaire and the assertions so unsupported by explicit evidence as to foster skepticism rather than acceptance. Yet, for all of this, students of technology will find this book rewarding reading.

In the first place, the author manages to spell out in unmistakable

terms the essential linkage between a nation's military doctrine—the official conception of tactical application—and the actual course of mechanical development that took place there. Thus, for example, in nations such as France, where the tank was regarded as a battering ram for the infantry, emphasis was on increases in armor plate with little or no attention to mobility—speed, range, etc. On the other hand, where tanks were regarded as mechanized cavalry, primarily for long range reconnaissance, armor plate virtually disappeared and mobility took precedence. Advocates of the armored assault division, a combination of striking power and mobility using tanks in combination with several other arms, established an entirely different specification in which an armament of high velocity guns took priority over mobility, and armor plate ranked a poor third. This conception, epitomized by the German *panzer* divisions of World War II, the author champions (while minimizing and all but ignoring the recent advances in anti-tank infantry weapons such as light, jeep-mounted recoilless guns). Clearly, until one is fully aware of these fundamental differences in doctrine and their ramifications, the widely varying technological implications of armored warfare will tend to be elusive if not obscure.

While the relationship between official doctrine and the direction of technical development is elaborated in great detail as the central theme of Ogorkiwcz's book, he also points out, though rather more sketchily, the role played by a host of other factors in the evolution of tanks. Especially provocative are his insights on the difficulties encountered in contriving organizations to insure a continual flow of conceptual advances and a corresponding response in the field of design. Similarly useful are his shrewd asides such as the one suggesting that cost-consciousness can be an effective though unintended check on the inevitable tendency of designers toward complexity, or his observations on the wisdom of the U. S. Army's decision to concentrate its limited prewar funds on the development of soundly engineered components—tracks, transmissions, suspensions, power plants, etc.—a policy "fully vindicated by the mechanical performance of American tanks" in World War II. Not the least contribution of this volume is the stimulus it should give to the writing of a wide range of monographs, especially objective studies on the industrial aspects of weapon design and development, a field now seriously neglected.

I. B. Holley, Jr.*

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U. S. Army in World War II. The Technical Services. The Chemical Warfare Service: Organizing for War. By Leo P. Brophy and George J. B. Fisher. *From Laboratory to Field.* By Leo P. Brophy, Wyndham D. Miles, and Rexmond C. Cochrane. Washington, D. C.: Office of the Chief of Miliary History, Department of the Army, 1959. 2 Volumes. Pp. xix, 498 and xviii, 498. Illustrations and Tables. $4.00 and $3.50.

With the release of the second Chemical Warfare Service volume in March of this year, the books in the series *United States Army in World War II* now number 51. More are in the planning and preparative stages; the third and concluding volume on the Chemical Warfare Service, *Chemicals in Combat*, is nearing completion. The United States Army is to be commended in this project of bringing the history of World War II before the American public in well written, well illustrated, and reasonably priced books. It is obvious from the list of titles of the books already issued and of those in preparation that every branch of the Army and all fronts in the war are or will be covered.

The two books under review should be of particular interest to the readers of *Technology and Culture*, for they are the official history of a service closely concerned with science and technology. Although various journal articles have been and will be written on specific subjects, it is unlikely that any other book will be published in this area. In a sense, it is immaterial how we judge these volumes as they, in all probability, will be our only source for a broad picture of the Chemical Warfare Service. Consequently, I am pleased that I can report that these two volumes appear to cover the history of the Chemical Warfare Service thoroughly. In fact the casual reader may rightfully complain that the authors relegated too much attention to details at the expense of pace and interest.

The authors of these two volumes certainly must have been confronted with a tremendous number of detailed documents to sift, study, understand, analyze, and interpret. I was impressed with the considerable skill of the authors in organizing the facts collected. There is no need to be concerned in this history with deviation from or approximation to fact. These two volumes are outstanding in this respect.

On the other hand, the test of a history is its relevancy to the stream of life not its reservoir of facts. Because science and technology are in a state of maturity, we should expect its history to have a form similar to a tree rather than like a collection growing item by item. It is in the area of relationship and evaluation, the interpretation of lessons from experience, and the correlation between progress in science and progress in waging war, that fault may be found with these two books.

Germany's use of gas in World War I made it necessary for the United States to organize for this new type of warfare first through the Bureau of Mines, then through the Gas Warfare Organization, and finally through the establishment of the Chemical Warfare Service in 1918. The vicissitudes of CWS from its establishment, almost demise at the end of World War I, skeletal existence until the rise of Hitler's war machine, mobilization for World War II, and responsibilities and functions during World War II are the subjects of the first volume, *Organizing for War*. This volume is more concerned with the organizational, administrative, and mobilization aspects than with the symbiosis of science and technology with chemical and biological warfare. Within these aspects, the authors intelligently inquire into the strengths, weaknesses, problems, and requirements; they make a strong case for future intensification of effort in several areas where CWS must remain in contact with the mainsprings of technological progress and administrative techniques and skills.

The second volume, *From Laboratory to Field*, traces the research, development, procurement, and distribution programs through which the armed forces were provided with toxic agents, flame throwers, incendiaries, smoke generators, gas masks, and the rifled chemical mortar. The authors missed a golden opportunity in this volume, in discussing research and development programs and industrial facilities, to relate CWS with the national and world environment. As a consequence, the reader tends to be overly critical of objectives and accomplishments unless he remembers and appreciates the state of knowledge and administrative skills then extant.

It is quite obvious that no organization can make a deep thrust into new territory without a momentum of research effort behind it. And this was the situation with CWS at the outbreak of World War II. It was without a research and development program, but, in all fairness, so were many chemical companies up to that time. The world was not enjoying an age of science; rather science and technology were developing at a pedestrian pace. It was the impetus of World War II that thrust the world, and in particular the United States, into the new age of science with speed and urgency. We may be thankful that the United States through its educational philosophy and industrial maturity was more ready for this age than were other nations.

This may not be the situation next time. The merit of these two volumes is that they present us with the opportunity to learn the lessons of history well. CWS must be given the wherewithal to maintain a momentum of research effort in chemistry and in biology. For its part, CWS needs to be increasingly responsive to the advances in science and technology. And, finally, the public must see to it that

the scientific manpower that dug trenches during World War I and foxholes during World War II is utilized effectively in achieving the objectives of CWS and other agencies.

HERMAN SKOLNIK *

Elihu Thomson: Beloved Scientist. By David O. Woodbury. Cambridge: Harvard University Press, 1960. Pp. xv, 358. Illustrations. $6.

This is a new edition of David Woodbury's well-rounded, smoothly written biography of a pioneer in heavy-current electricity. As M. I. T. Board Chairman J. R. Killian, Jr., notes in the preface, the book should appeal to a "new generation of youth." It should also appeal to the general reader seeking the diversion of a Horatio Alger story of substance. The historian, however, must approach the only available biography of a significant figure with caution.

There is little reason to doubt that Thomson was "beloved." Woodbury notes the numerous testimonials to Thomson, who seems to have assumed the character of an elder statesman of American technology. Furthermore, Thomson must have been respected by his professional colleagues, judging by the impressive list of scientific decorations and awards he received and by his being named to serve as acting president of M. I. T. after the First World War.

On the other hand, what historian would not view suspiciously Woodbury's interpretation of Thomson as a man who never "had an enemy in the world"; who had "the respect reserved for history's greatest humanitarians"—"a man without blemish, against whom none wished to voice criticism or calumny." If Thomson's relations with his early partner Edward Houston are fairly described, then this reviewer believes he knows where to look for evidence of at least one enemy. No one familiar with the broad sweep of human history could possibly characterize this good man as one of history's great humanitarians. While it is a petty thing, Thomson's behavior toward cats—described by Woodbury—at least opens the question of whether Thomson was without blemish.

Thomson has an established place as a pioneer inventor in electrical apparatus, but Woodbury would elevate him to the rank of Faraday, Kelvin, and Maxwell. His invention of resistance welding is of un-

* Dr. Skolnik, Manager, Technical Information Division, Hercules Powder Company Research Center, Wilmington, Delaware, has had an active interest for many years in the philosophy and history of science, particularly that relating to chemistry. Among his many publications in chemistry, several have been concerned with the historical aspects. He is editor of the *Journal of Chemical Documentation*, an American Chemical Society publication.

deniable significance and his early arc-lighting system won respect and acceptance. When it comes, however, to accepting Woodbury's contention that Thomson conceived of the alternating current-transformer system ("the germ of future electric power systems"), the historian must observe caution. Is it pure coincidence that Thomson began intensive experimenting with alternating current and transformers after visiting the Paris exhibition in 1878 where Gramme dynamos supplied alternating current to the electric candles of Paul Jablochkoff—the same Jablochkoff who had patented a distribution system using prototransformers a year earlier? According to Woodbury, Thomson dismissed the display on the Avenues de l'Opéra as "mostly a stunt" and found the exhibition a disappointment. When it comes to accepting Woodbury's statement that his hero anticipated Edison's subdivision of electric light by fully a year, the historian can simply dismiss this as a confusion of terminology.

Historians of science will certainly challenge the label of scientist being applied to Thomson—it is unfortunate that "beloved applied scientist" makes such an awkward phrase. Thomson's courageous experimentation with X-rays was truly admirable and the prestige he lent to efforts to standardize electrical units commendable, but was his enthusiasm matched by scientific achievement?

Finally, it should be emphasized that *Beloved Scientist* is a skillfully and warmly done biography and as such is likely to win friends for the history of technology. It does not, however, do full justice to an "inventive genius, engineer, . . . and educator."

THOMAS P. HUGHES *

The Modernization of Iran, 1921-41. By Amin Banani. Stanford: Stanford University Press, 1961. Pp. ix, 191. 8 Plates. $5.

Iran today is involved in a third historic phase of economic growth, a process which she hopes will this time transform her feudal poverty-stricken village economy (almost unchanged since Cyrus' day) into a literate, technological society in which the individual will enjoy material security and the possibilities for creative living.

The first phase of this growth took place during the nineteenth and early twentieth centuries when a few sons of the landed aristocrats were sent to Western Europe to learn the military, administrative, professional, and technical skills of complex government. In return, a few specialists from France, Switzerland, Germany, and the United States were invited to train government administrators, fiscal experts, and

* Dr. Hughes, associate professor of history at Washington and Lee University, has written on the development of the electrical industry.

technicians in Iran itself. (Russia and Great Britain were not included in the invitation. At this time, both looked upon Iran much as a cat does a canary.)

The second phase was directed by Reza Khan (later Reza Shah), an army officer who took over political power in 1921 and held it until 1941 when he resigned under the combined pressures of the Soviet Union, Great Britain, and the United States.

The third phase is the uneven trend toward modernization from 1941 to the present which is being continued by his son, the present Shah Reza Pahlavi.

Banani's book touches briefly upon the historical background of the first phase, sketching a picture of the ferment among Iran's educated elites-in-exile for a modernization which would remake the country into the nineteenth century image of the Great Powers of the West. Part of this restlessness evidently arose from the humiliation felt by Iran's military weakness vis-à-vis Great Britain and Russia. Part may also have arisen from a genuine admiration for the scientific and technological progress developed in the West from the seventeenth century onward.

The cultural change involved in the achievement of a technological society in the underdeveloped areas of the world requires an enormous investment of capital to train personnel. This means that the tiny elites in control of the new and old nations must be sufficiently motivated to make the social and economic sacrifices necessary if an ever-growing number within a population are to acquire the technical, commercial, administrative, scientific, and industrial knowledge which can spell the difference between poverty (with its concomittants of illiteracy and disease) and human dignity (with its twin of security). In Iran, the land-owning aristocrats are sympathetic to the goals of modernization but would prefer that the sacrifices be made by others. They still wish to be left alone to enjoy the life of a leisured aristocracy and, for the most part, are not yet willing to assume the risks and responsibilities of becoming industrialists, officials, or even large-scale merchants. It is possible that in the future the motivations for change may come primarly from the frustrated sons of the lower middle classes in the military or in government service (as in Egypt and elsewhere). In this respect, Iran seems to be behaving as the norm rather than the exception.

The steps involved in changing a marginal village economy to one that is urban and industrialized need to be defined theoretically. Though this was not Banani's intention, his book is nevertheless a contribution to the small but increasing collection of historical accounts dealing with the cultural effect of the scientific and technological movements of our day on the various underdeveloped areas of the world. We now need another book which will take us to 1961 in

Iran, plus still another which is concerned with how, what, and by whom technological innovations are incorporated into the cultural fabric of Iranian society.

DAVID RODNICK *

Nationalized Industry and Public Ownership. By William A. Robson. Toronto: University of Toronto Press, 1960. Pp. 544. $7.50.

The author, Professor of Public Administration in the London School of Economics and Political Science (University of London), set out to present a "reasonably balanced" view of the highly controversial subject of public ownership and the successes and failures of the British experience with nationalized industry. The "balance" here appears to be between extremes of thought as represented by those who agree in principle on the desirability of the government's direct participation in management and control of industry in order to further the aims of social and economic justice but who disagree as to the methods to be used. With the exception of sometimes appearing to confuse the effect of changes in organizational structure, due to a rational consolidation of operating entities, with the virtues of nationalization, he has attained his aim. (To say that the performance of nationalized electric and gas industries is better than the prior form of independent units is as much a case for the establishment of monopoly as it is for public ownership.)

The volume is by turns a history of the nationalized industries of Great Britain, an examination of the theory and problems connected with the operations of such industry by public corporations, an inquiry into the criticism and performance of such industries and corporations, and an attempt to formulate clearly the grounds for both the evaluation of the performance of nationalized industries and the determination of their ultimate role in the political economy. Any such program of necessity shows the dependence of technology on culture but does so indirectly in this case.

The major emphasis is on reporting and evaluating the success and failure of the public corporation. As a constitutional innovation, the public corporation is designated by the author as "the most important invention of the twentieth century in the sphere of government institutions." Its significant feature is that the principle of regulating in detail the activities of employees has been abandoned; thus, if a public

* Professor of sociology and anthropology at Iowa Wesleyan College, Dr. Rodnick has been concerned with the study of culture change for more than 25 years. He has done extensive field work in the United States, Germany, England, Czechoslovakia, France, Italy, Norway, India, and Japan; and has travelled in Lebanon, Israel, Iran, and Pakistan.

corporation does adopt a bureaucratic structure it is not because of legal compulsion to do so.

Professor Robson's attempt to assess the nature and extent of "success" or "failure" in the performance of various public corporations displays the difficulties inherent in a situation where the needed criteria are lacking. A public corporation can be measured from three directions, and evaluation involves the creation of some index based on a mixture of the rationales connected to public ownership, public accountability, and business management. This index depends on opinion as to what combination of rationale does best serve public ends. Men have been known to disagree on far less muddled matters.

There is in the author's approach a normative element in that he appears sure that there are criteria for determining what is "right," and that when this condition is attained certain difficulties will vanish. But because his approach is pragmatically socialistic rather than dogmatically so, the defect is one which will not harm the reader and does give the author a flexibility of position. Adopting his use of the normative orientation, we can say that his volume provides ample evidence for his observation: "The right balance between independence and political control has not yet been struck in most countries, and the process of search and adjustment is still continuing."

To students of technology, the main interest of the book must lie in the information provided about some aspects of the operation of public corporations and in the questions raised as a result. These matters are also at the heart of the drastic revision of attitude on the part of both the British Labour Party and the trade union movement toward nationalization since its early days. But the British experiences have a wider importance inasmuch as state intervention in the economy in the direction of molding technology and performance appears to be on the increase.

The book makes it evident that the problems arising out of monopoly are not essentially changed when that monopoly is publicly owned rather than privately owned. There appears to be some connection between technological advancement and the size and nature of the organization through which this advance is expressed and the total environment in which the organization operates. Included in this last are aspects of its past history.

If mankind is deliberately to engage in a program aimed at stimulating technological progress more attention must be paid to the matter of "optimum size." This is not so much a matter of statistics as a determination of *what* is to be measured, along with a ranking of values. What is optimum size in the way of financial resources for an undertaking or risk is not optimum in the same way for engineering performance, but these are the less complex questions. The determination of the optimum conditions under which enterprise and initiative

might be said to flourish, or suffer the least handicap, is the complex matter, especially when the size of the administrative unit is also a factor.

The balancing of the effect of a given size of firm on all these varieties of "optimums" is the chief problem of management in private enterprise; it also appears as a critical problem in public enterprise insofar as British experience to 1960 is concerned. It would seem that in theory, at least, the removal of budgetary restrictions which are a concomitant of operation on the "profit motive" should give an advantage to public enterprise. That it does not do so must be laid in part to the inability to solve the other "optimizing" difficulties.

It is to Professor Robson's credit that he sees that the decentralization vs. centralization debate can only be resolved in terms relating the degree of centralization to the size of the administrative unit, and that, from the standpoint of performance, the attainment of the correct size and shape of the operating unit is as important in public enterprise as it is in private enterprise. It is unfortunate that little information is provided about the performance of sub-units within nationalized industries, because they apparently do differ in size, and such information might provide a clue for improving the general frame of reference used in evaluation.

The British experience has a value in suggesting new lines of approach, and Professor Robson is keenly aware of this. Among other things, he urges the Labour Party to abandon its demands for total nationalization of the steel industry, which was denationalized upon the return of the Conservatives to power, and to substitute the policy of partial nationalization on the grounds: "If total capacity is insufficient, the nationalized firms can expand by building new plants. If prices are too high, they can reduce theirs. If technology is lagging, they can introduce whatever improved processes are available." It is obvious that such strategy would greatly simplify the matter of evaluating performance of such publicly owned individual concerns and remove the issues which are connected solely with the monopoly status. Such a narrow construction of the aims of government control would also serve to remove the thorny problem of promoting "economic and social justice" from the evaluation of performance.

The world, undoubtedly, would be a different place if men by nature automatically sought improvement; for one thing, the concern with modifying economically significant behavior by altering the dimensions of rationality would be far less important. It is this problem, that of changing behavior by changing the dimensions of rationality, which is the real source of the difficulty encountered in the British effort to combine public ownership, public accountability, and business management in a successful way in the program of nationalizing industry. There are limits to what can be done here because of the need to maintain a clear-cut relationship between the desired behavior and individual

pay-offs valued in the existing society. Unquestionably it is much more difficult to get people to change their value systems than it is to adjust to new ways of obtaining those pay-offs. The insights of behavioral science are needed in economic reform programs.

It is this reviewer's opinion that Professor Robson's volume might have been improved if he had made explicit the theories of human behavior on which both the hopes and criticism of performance ultimately rest. It is over the validity of such theories that much of the debate about the virtues and defects of public ownership vs. private ownership is carried on, and only secondarily on the matter of whether the profit motive or the public service motive works best. This sort of approach might have thrown light on what the author early (p. 21) in his book points out: "Some of the most difficult problems which have arisen . . . in the sphere of nationalization are due to the fact that the technology, organization, management, labour relations, and *morale* of the industries are largely influenced by the history and conditions which prevailed when the industries were under private enterprise. One of the most important distinctions in the realm of public enterprise is between industries which have been taken over at a late stage of their development and those which have grown up as public undertakings."

It turns out that this last distinction has a methodological significance but, insofar as Professor Robson is concerned, one useful only for political reasons. This is unfortunate, since one of the major problems in any nation might well be the one of arresting decline in older industries on the basis of the kind of activity which non-profit motivation might make possible. But it appears that such decline is not to be readily cured by a change in investment goals or in rationalization of operating goals. Robson is quite clear in putting the responsibility for the decline on actions of private enterprise, yet in light of his later remarks the case for his convictions is weakened. "We know," he writes, "from our experience of coal and railways how hard it is to overcome a momentum of decline in an old industry." And then he notes: "To add to the number of such backward, declining, or stagnant industries in the public sector would almost certainly discredit the principle of nationalization beyond hope of recovery."

It seems clear that what the author attempted to do was to erect a new foundation for supporting nationalization using the experiences of the British experiment. In his preface he states: "I have never wavered in my belief that nationalization will not by itself bring about drastic changes in the operation of old established industries, although it may enable improvements to be subsequently made." In his Conclusion he writes: "We should . . . always bear in mind that the most favourable conditions for public enterprise are likely to exist in new or expanding industries or services; and the least favourable conditions in

old, declining, or stagnant branches of the economy." This would indicate that the English proponents of modern socialism are at least as "hard-headed and practical" as those who direct private enterprise.

EDWARD WISNEWSKY *

Central Planning in Czechoslovakia: Organization for Growth in a Mature Economy. By Jan M. Michal. Stanford: Stanford University Press, 1960. Pp. xiv, 274. $5.75.

Mr. Michal, a consultant at the Stanford Research Institute, has written a fine book which answers—and raises—many questions about its subject. The book, *Central Planning in Czechoslovakia*, combines several functions: it is a description of and a general statistical compendium on the Czechoslovakian economy; it is a comparison of that national economy with other economies, both market and centrally planned; and, finally, the book contains material on the problems of planning in Czechoslovakia in particular, and in other such economies in general.

The author's wide selection of statistics indicates many interesting facts. For example, the building industry seems very inefficient, construction requiring three to four times longer than in the West. Agriculture also is a weak spot, with 1958 production below the pre-war level. On the whole, however, the Czechoslovakian output rates, productivity, and technology are comparable with those of other highly developed European nations. In terms of the overall standard of living, we come upon important but intangible value judgments and internal institutional changes which render many comparisons meaningless.

Included in the general descriptive material are figures which point up a surprisingly high level of foreign aid to other Eastern European nations, and also to Asian and African states.

Using the book's title, and my own interest, as a guide, the preceding material serves as background to the chief problem—planning. From this extensive background we can see that the men and institutions who make the plans in Czechoslovakia have one great advantage as well as one great disadvantage, not faced by their counterparts in other planned economies. Their advantage is that they are dealing with a mature economy which furnishes them with the tools of a technology and production figures which can stand comparison in many

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cases with those of, say, France or Germany. No tremendous human sacrifices are called for to construct a modern industrial society out of an undeveloped backward peasant economy. Nor is the threat of imminent mass starvation or general breakdown of the economy, as was the case in post-civil war Russia, facing the planners.

On the other hand, the planners' disadvantage is that they must deal with problems unique and complicated. They must make decisions, thousands and thousands of them, which in a market economy are made, however imperfectly, by market forces. Mr. Michal stresses this difference between the market economy and the centrally planned economy as crucial. He feels that because of the absence of a market, even a market of imperfect competition, the pricing system is basically artificial. This artificiality leads to inconsistencies which compound themselves in the national account system and finally result in poor, one might say necessarily poor, planning decisions. The author points out some examples in describing cases of overexpanded and unemployed capital assets.

Of particular interest to the readers of *Technology and Culture* are Mr. Michal's views on the methods that planners employ to decide on investment alternatives. He writes at one point: "It would require a special study to examine the investment policy of Czechoslovak planning authorities." However, he does give some of his views on this matter; because of the absence of domestic market indicators, he feels that trends in the Western economies must be studied and used as a guide by the planners. In addition, Mr. Michal indicates situations where purely political needs dictate the decisions.

Given only this material and these views on planning, we are almost driven to the conclusion that a planned economy must find it impossible to keep up with other modern economies, let alone come forth with technological innovations. This conclusion would certainly seem presumptuous in the face of evidence in news reports from Eastern Europe.

I feel also that Mr. Michal's basic criticism of the planned economy is not developed enough. His criticism is not a new one. One of its earliest proponents was Professor Mises. In turn Mises' views were discussed and criticized by Oskar Lange in his essay "On the Economic Theory of Socialism" (reprinted in a book of the same title, edited by Benjamin E. Lippincott, University of Minnesota Press, 1938). Lange felt that a consistent internal accounting system was possible in a planned economy, and in fact, such a system would be more efficient than in a market economy. (Since writing this essay Lange has had considerable experience in the hard practicalities of the post-war Polish economy, and it would be interesting, of course, to have his views brought up to date). While Lange's pre-war theoretical essay should not outweigh a close study of the actual record of per-

formance of a planned economy, such as Mr. Michal has given us here, I do feel that Mr. Michal should have included some discussion of these theoretical points raised by Lange and, I would imagine, by Czechoslovakian economists since then. Such a discussion, along with the needed study of the actual process of making investment decisions from the viewpoint of the planners, economists, accountants, and even politicians, would be most valuable in helping us to understand the dynamics of the centrally planned economies, and also possibly, some of the planning which goes on within most market economies.

My final point is on Mr. Michal's statistics. I believe a weakness exists here in having to treat each nation and its economy exclusively as homogeneous units. Uneven developments within Czechoslovakia (Bohemia, Moravia, and Slovakia), as within most other nations, deserve more than the very brief mention they get.

At one point Mr. Michal states that Czechoslovakia enjoyed the third highest per capita energy consumption in Europe in 1958. He was, I believe, referring to electrical energy, and I also believe his statement is incorrect, as would be seen if similar data were included for Norway, Sweden, and Switzerland. This, though, is a minor question compared to the very valuable accumulation of statistics and descriptive material that Mr. Michal has given us. I hope that he will continue to explore this field further.

EDMUND M. BLEICH *

The Crisis We Face; Automation and the Cold War. By George Steele and Paul Kircher. New York: McGraw Hill Book Company, 1960. Pp. 220. $4.95.

Mr. Steele is reportedly a private computer consultant and Mr. Kircher is Associate Professor at the School of Business Administration at the University of California at Los Angeles. The authors attempt to mesh their two approaches in an effort to solve the world's problems. The importance of this dual background, at least as far as the reader is concerned, is that the authors have tended to write for two separate audiences. Presumably the engineer will be better able to evaluate the sections written by Mr. Steele and the social scientist better able to evaluate the sections written by Mr. Kircher. The overall integration and coordination which they stress as being so important for our technology is only partially present in this volume; it reads in parts almost like a series of unrelated newspaper editorials.

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From the viewpoint of the reader of *Technology and Culture* we shall attempt to summarize what we understand to be the essential points made by Steele and Kircher.

a. U. S. technicians and/or engineers and/or scientists tend to overdesign our technological devices. The authors are largely concerned with military hardware (and only incidentally with civilian hardware) of types that should operate with a minimum of human guidance. (They use the word "automation" continuously, a word which this reviewer would prefer avoiding.)
b. This overdesign, or overcomplexity, leads to interminable break downs of equipment. The more nearly completely we "automize" a machine by overdesigning it, the more maintenance difficulties we have.
c. The military hardware (and also civilian hardware) is becoming more and more complex and overdesigned with each passing day; the maintenance, equipmetn in turn becomes more complex until "2nd degree" maintenance machines are needed to keep the "1st degree" maintenance machines in operation so that they can maintain the original hardware in operating condition. This process of cancer cell type growth can continue indefinitely until we have a technology completely incapable of action. The two authors claim that this process is already affecting the efficiency of our defenses.
d. The essential reason for this overdesign is the method of procurement whereby innumerable sub-contracts for sub-designing as well as sub-manufacturing are let. Each sub-contractor tries to design his own part to perfection with no regard for the parts being designed by anyone else. This happens since no single body endeavors to draw adequate overall plans which will relate the various parts to each other. Without knowing exactly how a given part is expected to relate to others, obviously it is impossible to design that part properly.
e. A secondary set of reasons for this overdesign stems from the excess requirements stipulated by the government purchasing agents.

This reviewer cannot pass final judgment on the question of overdesign. We only know that other--presumably qualified technicians--have claimed that one part or another of our technology is overdesigned. The most recent instance which has come to our attention is the statement by Admiral John M. Will that "overdesign of ships was needlessly limiting the Merchant Marines' growth and competitive position" (*N. Y. Times*, June 11, 1961). On the basis of our purely personal experiences we feel that much of our current consumer tech-

nology—automobiles, lawn mowers, knife sharpening equipment, and even desk calculators, have more than enough devices built into them; and the more such built in devices the more servicing is likely to be required.

When it comes to military hardware we can only suppose—following our experience with civilian hardware—that overdesign and maintenance problems could be present. On the other hand, when considering such a machine as a vehicle which should reach the moon and return, we have no idea of how complicated it may really have to be in order to accomplish its mission.

As for the sub-contracting phase as the important reason for this overdesign, we have had experiences only in the social sciences. Here it is clear to us that the letting of sub-contracts and sub-design work without a rigorous overall plan is not conducive to maximum efficiency. For example, if a sample survey is to be conducted one cannot subcontract the several parts—determination of the sub-areas to be studied, writing of the questionnaire, drawing of the sample, conduct of the field work, preparation of the statistical tables, and analysis of the data—and end with a decent report. Only if a precise and efficient overall plan is drawn, in which all of the parts are interrelated, can the work then be sub-contracted—sometimes.

We can believe that this problem of overall planning and subcontracting can apply to every field of human endeavor.

The authors argue that we do so much sub-contracting because of the nature of our administrative machines which goes in heavily for division of labor, and a minimum of thinking which should lead to coordinated planning. With this view we are inclined to agree. Homo sapiens has never wasted time and effort on thinking if he can avoid it.

Finally, and according to the authors, the main reason for writing this polemic is their twofold argument that:

(a) Russians are not guilty of any of this overdesign and subcontracting leading to inoperative equipment, and

(b) that we must devote all our efforts for an indefinite time into the future, in constructing a Maginot line of 100 per cent automated equipment, to ward off the Russians.

The reviewer knows nothing about what the Russians may or may not be doing. We know only that we do not wish a society whose energies are directed exclusively to war and the preparation for war; there must be a better way for homo sapiens to pass his brief stay on earth.

A. J. JAFFE *

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Leisure in America: A Social Inquiry. By Max Kaplan. New York: John Wiley & Sons, 1960. Pp. xii, 350. $7.50.

More leisure time for more people and revolutionary new forms of leisure activity are among the major social consequences of modern technology. It is not surprising therefore that leisure has become the subject of serious, scholarly inquiry in which much hard work has been invested. In the present instance, Dr. Kaplan has sacrificed or used his own leisure (I assume work on this book was not part of his official duties as Director of the Arts Center of the Boston University School of Fine and Applied Arts) in order to explore such issues as:

> What is leisure? What factual trends in its use are observable? What are the factors in American life that affect these trends? Can a classification be developed that will go beyond the mere listing of activity patterned or structured in groupings and in time? How is it chosen and modified?

To this endeavor, Dr. Kaplan has brought many of the concepts, some of the methodology, and too much of the jargon and pedantic writing of modern sociology and psychology. The result is a partial illumination of many important facets of this multi-faceted subject in a book which brings together but does not always successfully relate much interesting information on the state of leisure in America.

Starting from leisure as a "construct" with seven distinctive elements, Kaplan proceeds to an analysis of the relation between leisure and a number of variables which condition the nature and quality of leisure activity. These are: (1) *work*, which has a large though gradually lessening influence on the forms of leisure activity; (2) *personality traits*; (3) *family*, with considerable attention to differences among the numerous functions and influences of leisure in family life; (4) *social class*, which is found to be an outdated concept when the pattern of American leisure is examined; (5) *subcultures*, inadequately treated in terms only of Negro and Jewish minorities; (6) *community*, the differing patterns of leisure in rural and urban situations; (7) the *state*, in its role of encouraging and creating conditions for leisure with the consequent problem of control; (8) *religion*, both teachings on work and leisure and the role of churches in a leisure-oriented society; and (9) *value systems*, with the analysis shifting from conditions of leisure to evaluation of them.

Turning to another set of categories, Kaplan next classifies leisure activities into six types and then raises the question of the dynamics which determine in any given situation which among alternative activities an individual chooses. Seven forms of control are identified and discussed, and to these are added analysis of the roles available in our culture.

In a final chapter of evaluation of the quality of leisure in America,

all of the author's basic optimism, hovering beneath the surface throughout the book, comes to the fore. One value by which leisure is judged is "creativity," and it is Kaplan's view that the American people have been and are now moving in the direction of increasingly creative uses of leisure. As he admits in the preface, his attitude is one of

> greater respect for the masses than is currently shown by the younger crop of social scientists, who (in my own view) have unwittingly been defending the values of a departed aristocracy and a feudal way of life.

Following out this judgment, he places more faith as indicators of trends in the thousand community orchestras in America than in the evidence of status seeking, conformity, or the high ratings of TV westerns. This perspective avoids the worst of the anguished self-criticism of so many commentators on the American cultural scene, but at the same time it leads to an air of detachment from inner processes of American culture which gives this study a rather artificial and external character. There are too many categories and classifications and not enough resolution of them into the living vitality of a nation at leisure.

And even the categorization is somewhat suspect when it is found to have omitted one of the major institutions affecting the nature and quality of leisure. There is almost no mention of education as itself a form of leisure activity and none of the role of education as a major influence on the style of leisure. Certainly it rates a place among the variables listed above. Indeed, if one thinks of the need to cope with the problems of increasing leisure, the role of education is paramount. "Liberal" education is, after all, the education of the "free man"—the man freed from the necessity of devoting all his energy to survival. If we are to achieve "the fullest development of creative values in the new leisure" much of the burden will fall on education. It must therefore increasingly become a preparation for a life of leisure more than of work.

Whether we are to have the opportunity to grow in our capacity for creative leisure depends in the first instance, as the author suggests in his preface, on whether we survive at all. Given this acknowledgment of the conditions in which we live, it is surprising that Kaplan says nothing about the impact of our vast military establishment on the forms and values of leisure. By ignoring this major influence in our culture, he misses the opportunity to discuss the fascinating problem of the relation between leisure and war. To what extent, for example, can war itself be classified as a form or perversion of leisure activity? Is it perhaps a sign of the absence or breakdown of institutions for channelizing creative energies into less destructive patterns? To what extent today do the patterns of leisure activity in America reflect a desire to escape the pressing issues of our time?

To suggest such questions is merely to indicate the broad implications of the subject of this book. But, as the author himself says in his conclusion, the inquiry into leisure is only just beginning, and as a beginning, the book is useful, both for the information it contains and for the problems it suggests for further study.

NORMAN D. KURLAND *

Psychophysiological Aspects of Space Flight. Edited by Bernard E. Flaherty. New York: Columbia University Press, 1961. Pp. xii, 393. $10.

Less than twenty years ago, through the intensive and cumulative achievements of science and technology, mankind triumphantly hailed the arrival of a new era, one in which atomic science promised potential benefit to humanity despite its initial destructive use. Today we are in the midst of still another vigorous and competitive technological effort, for within the last three years science has snatched us out of the Atomic Era and thrust us into the Space Age. Such rapid progress not only taxes man's understanding, exciting the imagination and inspiring myriad fantasies about what the future holds, but also raises specific questions about man's destined role in such an age. The rockets are ready; what about man? Paraphrasing Samuel Walter Foss, Don E. Flinn says (p. 87), "Bring me men to match my missiles," and it is to this subject—man's preparedness for space travel—that this book is devoted. It examines human psychology and physiology as they relate to space flight so that man may be trained and equipped to withstand its rigors and provision may be made to correct for his limitations.

The contents of this book, which pre-date this year's spectacular manned space flights, are the product of a conference, probably the first of its kind, held at Brooks Air Force Base on May 26-27, 1960, under the auspices of the School of Aviation Medicine. As has become customary, this conference was an interdisciplinary one, attended by scientists of many backgrounds and interests. Forty-four people reported their researches and opinions in the 29 papers which make up the four sections of the book: (1) Technical Background and Experience, (2) Critical Problem Areas, (3) Problems of Human Reliability, and (4) Special Techniques of Control. At the time of the conference more than 300 animals had been sent to the edge of space, and although research has ultimately to deal with man himself and his

* Dr. Kurland is Associate Director of the Inter-University Committee on the Superior Student. He has written of the impact of advancing technology on leisure time.

reactions to space travel, these tests have provided much preliminary data useful in anticipating the psychophysiological problems which space flight will create.

The discussion covers many potential problems of space flight: the equipment in which the astronaut will have to function, his need not only to survive within his equipment but also to manipulate it, and the process of selecting and training an astronaut. The stresses to which he will be subjected receive particular attention; he will be undertaking a great new adventure with an unpredictable outcome, and he will, moreover, face certain more readily determinable sources of trouble: acceleration and deceleration, shock, escape from the space capsule, acute temperature changes, radiation and magnetic field, and—a new experience for man—weightlessness. How well will he do under these circumstances? Can he be adequately trained and maintain his training? What motivates people interested in making such flights? Does man have a physiological cycle which might affect his survival? Is there any danger of sensory over-loading? How will man react to total isolation? And to potential sensory deprivation? Might such conditions confuse him, making him incompetent to perform his assigned functions, and thereby lead to errors in control which could be catastrophic? Looking ahead to a time when our rockets will be larger, there is also a discussion of interpersonal relations in isolated small groups.

This book closes with a group of articles on theoretical ways of altering man's normal state so that he may function more comfortably and appropriately on space flights. For example, could he be put into a state of relative hibernation for long trips through the use of hypothermia? Or would hypnosis be of value? Is there some way that man could be fed, at an appropriate rate, measured dosage of some medication to help him survive the rigors of space travel?

The imminent conquest of space has excited everyone's imagination. This book, although too technical in parts for a layman's easy reading, is exciting too, for it deals with many of the probable initial problems of the first astronauts. This volume is almost certainly the first in this particular area, and, although in time its material will undoubtedly seem rudimentary, at the moment it is very alive and very pertinent.

ERIC T. CARLSON *

* Dr. Carlson is Assistant Professor of Clinical Psychiatry at Cornell University Medical College and head of the departmental sub-division of the History of Psychiatry at Payne Whitney Psychiatric Clinic, New York Hospital. His researches have dealt with the history of psychiatric thought, particularly in the United States.

Organizational Notes

The 1961 meeting of the Society for the History of Technology will be held in Washington, D. C. on December 28-30. A complete program will be mailed to members in November, but Dr. Thomas P. Hughes of the Program Committee would like to make known the tentative plans for the meeting.

FOURTH ANNUAL MEETING

Shoreham Hotel—Washington, D. C.

December 28-30, 1961.

Thursday, December 28

2:30-4:30 p. m. (Green Room)—Executive Council Meeting

4:30-6:00 p. m. (South Room)—Annual Business Meeting of the Society

Friday, December 29

10:00-12:00 a. m. (Club Room)—"Early Trans-Atlantic Science and Technology"

Joint session with the History of Science Society

Chairman: Whitfield J. Bell, Jr. (American Philosophical Society)

Speakers: John Sawyer (Williams College), "Blanc and Interchangeable Manufacture: France and America."
George F. Frick (University of Delaware), "Anglo-American Natural History Circle."
Norman B. Wilkinson (Eleutherian Mills-Hagley Foundation), "Brandywine Borrowings from European Technology"

Commentator: Edward Lurie (Wayne State University)

2:30-4:30 p. m. (South Room)—"Recollections of the Early History of Naval Aviation"

Chairman: Doyce B. Nunis (University of California, Los Angeles)

Speakers: Garland Fulton (Captain, U. S. N. [Ret.]), "The Early Days of Naval Aeronautics: A Technological View"
Charles J. McCarthy (New York City), "Aircraft Design in the Mid-1930's"

Commentator: John B. Rae (Harvey Mudd College)

5:00-7:00 p. m. (Tamerlane Room)—Social Gathering.

Saturday, December 30

10:00-12:00 a. m.—"The Role of Mechanical Technology Before World War I'

Joint session with the American Historical Association

Chairman: Leonard Carmichael (The Smithsonian Institution)
Speakers: Reynold Wik (Mills College), "The United States"
Cyril Black (Princeton University), "Russia"
Commentator: Robert S. Woodbury (Massachusetts Institute of Technology)

2:30-4:30 p. m. (North Room)—"Anthropological and Sociological Aspects of Technological Change"

Speakers: Margaret Arnott (University of Pennsylvania), "The Making of the Candle of the Dead at Salamis"
Absolom Vilakazi (Hartford Seminary Foundation), "Culture Contact and Technological Changes in Zulu Agriculture"
Francis R. Allen (Florida State University), "Social Effects of Sputnik"
Anthony Leeds (College of the City of New York), "Societal Complexity and Alleged 'Simpleness' of Technologies"

Headquarters of the Society's meeting will be at the Shoreham Hotel, and all the sessions will be held there, with the possible exception of the joint program with the American Historical Association.

The Shoreham Hotel is providing special rates for members of the Society: single room, $9; double room, $14. Reservations, mentioning the Society's name and indicating probable arrival and departure times, should be addressed to Mr. Philip Hollywood, Shoreham Hotel, Washington, D. C.

The Program Committee which has been responsible for arranging the sessions consists of Thomas P. Hughes, Paul Leser, and John Streeter. Ralph Sanders (Chairman), Eduard Farber, and Wilson Scott comprise the Local Arrangements Committee.

* * * * *

The Society for the History fo Technology is also co-sponsoring a program with Section L (History and Philosophy of Science) of the American Association for the Advancement of Science, which is meeting in Denver. This program, on the history of uranium mining, will be held on December 26 at 4 p. m. in Ballroom B of the Brown Palace Tower in Denver, Colorado.

Announcements

The University of Delaware, in cooperation with the Eleutherian Mills-Hagley Foundation, will award two Hagley Museum Fellowships in April of 1962 for the academic years 1962-1964. Recipients of these grants take graduate work in history and related fields at the University of Delaware. In addition, they spend half of each week during the academic year at the Hagley Museum, Wilmington, Delaware, where they receive training in museum work, and at the Eleutherian Mills Historical Library, where they conduct research. They complete their work, including a thesis, in two years, and graduate from the University of Delaware with a Master of Arts degree in American history. The program is of special interest to those who wish to study the development of American industry and technology.

Each fellowship carries an annual stipend of $1,800, and is renewable upon satisfactory completion of the first year. Applications should be received by March 5, 1962. For further details, address the Chairman, Department of History, University of Delaware, Newark, Delaware.

* * *

The Case Institute of Technology announces a graduate program in the History of Science and Technology, the first in the Untied States to deal specifically with the history of technology as well as the history of science.

The graduate program, leading to a Master of Arts degree, is under the direction of the Department of Humanities and Social Studies. In addition, courses offered by the faculties of science, engineering, and management will be open to qualified students wishing to focus their study on some aspects of these fields. Sufficient flexibility is provided by the program so that undergraduate training either in the liberal arts or in science and engineering can be both deepened and complemented at the graduate level.

Graduates of accredited colleges holding either the B. S. or B. A. degree will be considered for admission to the program. In general, students should have completed at least three years of mathematics and science, three years of history and social science, and two years of a foreign language as part of their undergraduate work; deficiencies in any of these may be remedied by taking their equivalents at Case Institute, and such deficiencies must be removed before a student can qualify for admission in full standing. Fellowships and assistantships are available.

Information regarding the program and application forms for admission and financial assistance may be obtained from Dr. Melvin

Kranzberg, Library-Humanities Building, Case Institute of Technology, Cleveland 6, Ohio.

* * *

The Verein Deutscher Ingenieure has issued the following announcement regarding the 1962 Conrad Matschoss Prize for writings in the history of technology:

CONRAD-MATSCHOSZ-PREISAUSSCHREIBEN 1962

FÜR TECHNIKGESCHICHTLICHE ARBEITEN

In unserer von der Technik wesentlich geprägten Zeit kommt auch der Technikgeschichte eine ständig wachsende Bedeutung zu. Der um sie hochverdiente langjährige Direktor des Vereins Deutscher Ingenieure, Professor Dr.-Ing. E. h. Dr. phil. h. c. Conrad Matschoß, hat immer wieder auch auf ihren besonderen Wert für die Ingenieurbildung hingewiesen. In diesem Zusammenhang hat er vorgeschlagen, sowohl technikgeschichtliche Arbeiten wissenschaftlichen Charakters zu fördern, wie auch die fü die technikgeschichtliche Forschung vielfact wichtigen Erinnerungen älterer Ingenieure nutzbar zu machen, sofern sie über das rein persönliche Erlebnis hinaus von weitreichender technikgeschichtlicher Bedeutung sind.

Als Anregung zu solchen Arbeiten veranstaltet der Verein Deutscher Ingenieure seit 1953 alle zwei Jahre das Conrad-Matschoß-Preisausschreiben. Hiefür stehen 3000 DM zur Verfügung, die ganz oder geteilt vergeben werden können.

1. Für das Conrad-Matschoß-Preisausschreiben können eingereicht werden:

   1.1 Arbeiten, die neue Erkenntnisse auf Grund eigener technikgeschichtlicher Forschungen bringen. Sie müssen wissenschaftlichen Ansprüchen genügen, Quellennachweise und Schrifttumsangaben enthalten. Der Umfang einer Arbeit darf (ohne Bilder) 20 Schreibmaschinenseiten (DIN A 4 zu je 30 Zeilen) nicht überschreiten.

   1.2 Arbeiten über eigene Erinnerungen an bedeutende Persönlichkeiten aus Technik und Wirtschaft und an entschreidende Ereignisse, die es wert sind, für die Technikgeschichte festgehalten zu werden. Das Manuskript einer solchen Arbeit darf 10 Schreibmaschinenseiten (DIN A 4 zu je 30 Zeilen) nicht überschreiten.

2. Jeder Einsender kann sich nur mit einer Arbeit in deutscher Sprache beteiligen, die noch unveröffentlicht ist und bis einen Monat nach der Entscheidung des Preisgerichtes nicht veröffentlicht werden darf. Die

mit einem Kennwort versehene Arbeit muß in zweifacher Ausfertigung in einem Umschlag verschlossen eingereicht werden.

LETZTER EINSENDETERMIN: 1. Juli 1962

ANSCHRIFT: Verein Deutscher Ingenieure, Düsseldorf, Prinz-Georg-Straße 77/79.

3. Der Umschlag, der die beiden Arbeiten enthält, muß außen den Vermerk: Conrad-Matschoß-Preisausschreiben 1962 und das Kennwort tragen. Der Name des Verfassers darf weder auf diesem Umschlag noch auf der Arbeit angegeben werden. Die Angaben zur Person des Verfassers (Name, Titel und Beruf, Gerburtstag, Anschrift) müssen in einem zweiten, ebenfalls verschlossenen Umschlag enthalten sein, der außen nur das Kennwort tragen darf. Beide Umschläge sind gemeinsam in einem dritten Umschlag an die oben angegebene Anschrift mit dem Vermerk: Conrad-Matschoß-Preisausschreiben 1962, ohne Angabe des Absenders, einzusenden.

4. Der Vorstand des Vereins Deutscher Ingenieure beruft das Preisgericht. Die Entscheidung des Preisgerichts ist unanfechtbar. Sie wird im Dezember 1962 in den „VDI-Nachrichten" bekanntgegeben und den Teilnehmern brieflich mitgeteilt.

5. Der zu Verfügung stehende Betrag von 3000 DM kann ganz oder geteilt oder auch nur teilweise zuerkannt werden.

6. Mit der Zuteilung eines Preises geht das Verlagsrecht an der preisgekrönten Arbeit auf den Verein Deutscher Ingenieure über. Eine Verpflichtung zur Veröffentlichung besteht jedoch nicht. Sofern auf eine Veröffentlichung verzichtet wird, kann der Verfasser seine Arbeit mit Einwilligung der Vereins Deutscher Ingenieure an anderer Stelle veröffentlichen.

7. Auch Arbeiten denen kein Preis zuerkannt wurde, können vom Verein Deutscher Ingenieure im Einvernehmen mit dem Verfasser veröffentlicht oder zur Aufnahme in das technikgeschichtliche VDI-Archiv erworben werden.

DÜSSELDORF, IM JULI 1961

VEREIN DEUTSCHER INGENIEURE

INDEX TO VOLUME II

OF

TECHNOLOGY AND CULTURE

(R) indicates Book Review.

Aeronautics and Astronautics: Dempster, *The Tale of the Comet*, (R) 291; Dryden, "Future Exploration and Utilization of Outer Space," 112; Emme, *Impact of Air Power*, (R) 75; Flaherty, *Psychophysiological Aspects of Space Flight*, (R) 433; Gartmann, *Rings Around the World*, (R) 37; Hubler, *Big Eight*, (R) 293; Rae, "Science and Engineering in the History of Aviation," 391; White, "Eilmer of Malmesbury, an Eleventh Century Aviator," 97.

Agriculture: Gates, *The Farmer's Age*, (R) 185; Kohlmeyer and Herum, "Science and Engineering in Agriculture: A Historical Perspective," 368; Rasmussen, *Readings in the History of American Agriculture*, (R) 183; Spence, *God Speed the Plow*, (R) 187.

Aitchison, Leslie, *A History of Metals*, (R) 166.

Aitken, Hugh G. J., *Taylorism at Watertown Arsenal: Scientific Management in Action, 1908-1915*, (R) 191.

American Building Art: The Nineteenth Century, Carl W. Condit, (R) 402.

The American Civil Engineer, Daniel H. Calhoun, (R) 61.

Anderson, Harold H., *Creativity and Its Cultivation*, (R) 89.

The Archaeology of Weapons: Arms and Armour from Prehistory to the Age of Chivalry, R. Ewart Oakeshott, (R) 171.

Armor: See also Weapons. Oakeshott, *Archaeology of Weapons*, (R) 171; Ogorkiewicz, *Armor: A History of Mechanized Weapons*, (R) 415.

Armor: A History of Mechanized Forces, Richard M. Ogorkiewicz, (R) 415.

The Atom and the Energy Revolution, Norman Lansdell, (R) 83.

Atomic Energy: Kramish, *Atomic Energy in the Soviet Union*, (R) 85; Lansdell, *The Atom and the Energy Revolution*, (R) 83.

Atomic Energy in the Soviet Union, Arnold Kramish, (R) 85.

Automation: See also Computers, Industrial Engineering. Steele and Kircher, *The Crisis We Face*, (R) 428.

Automobile: Diesel, *et al.*, *From Engines to Autos*, (R) 189; Greenleaf, *Monopoly on Wheels*, (R) 289.

Banani, Amin, *The Modernization of Iran, 1921-41*, (R) 420.

Barber, Bernard, (R), Hoselitz, *The Sociological Aspects of Economic Growth*, (R), 264; Smelser, *Social Change in the Industrial Revolution: An Application of Theory to the British Cotton Industry, 1770-1840*, 57.

Bedini, Silvio, (R), Eckhardt, *United States Clock and Watch Patents, 1790-1890*, 278; (R), Hoopes, *Shop Records of Daniel Burnap, Clockmaker*, 280.

Big Eight: A Biography of an Airplane, Richard G. Hubler, (R) 293.

"A Biographical Bibliography of Electrical Engineers and Electrophysicists," Part I (Books), 28; Part II (Periodicals), 146, Thomas James Higgins.

Birr, Kendall, (R), Steinmetz, *Vanishing Crafts and Their Craftsmen*, 407

Black Rock: Mining Folklore of the Pennsylvania Dutch, George Korson, (R) 408.

Bleich, Edmund M., (R), Michal, *Central Planning in Czechoslovakia: Organization for Growth in a Mature Economy*, 426.

Blue Collar Men: Patterns of Dual Allegiance in Industry, Theodore V. Purcell, (R) 411.

Boulding, Kenneth E., (R), Fourastié, *The Causes of Wealth*, 262.

Bredo, William, *Industrial Estates: Tool for Industrialization*, (R) 295.

British Shipping, R. H. Thornton, (R) 73.

Brittain, Robert, (R), Rasmussen, *Readings in the History of American Agriculture*, 185.

Brody, David, *Steelworkers in America: The Nonunion Era*, (R) 409.

Brochl, Wayne G., Jr., *Precision Valley: The Machine Tool Companies of Springfield, Vermont*, (R) 180.

Brophy, Leo P. and Fisher, George J. B., *U. S. Army in World War II. The Technical Services. The Chemical Warfare Service: Organizing for War*, (R) 417.

Brophy, Leo P., Miles, Wyndham D., and Cochrane, Rexmond, C., *U. S. Army in World War II. The Technical Services. The Chemical Warfare Service: From Laboratory to Field*, (R) 417.

Building: See Civil Engineering.

Burger, Henry G., "Technoculture," 260; (R), Bredo, *Industrial Estates: Tool for Industrialization*, 295.

Burlingame, Roger, "Technology: Neglected Clue to Historical Change," 219.

Burstall, Aubrey F., (R), Clark, *The Development of the English Traction Engine*, 288.

Calhoun, Daniel H., *The American Civil Engineer*, (R) 61.

Cameron, Rondo E., *France and the Economic Development of Europe, 1800-1914*, (R) 403.

Carlson, Eric T., (R), Flaherty, *Psychophysiological Aspects of Space Flight*, 433.

Carlson, Robert E., (R), Fogel, *The Union Pacific Railroad: A Case in Premature Enterprise*, 405.

Carmichael, Leonard, (R), Anderson, *Creativity and Its Cultivation*, 89.

The Causes of Wealth, Jean Fourastie, (R) 262.

Central Planning in Czechoslovakia: Organization for Growth in a Mature Economy, Jan M. Michal, (R) 426.

Chang, Tien S., (R), Needham, *The Development of Iron and Steel Technology in China*, 176.

Charleston's Sons of Liberty: A Study of the Artisans, 1763-1789, Richard Walsh, (R) 59.

Chemistry and Chemical Engineering: Brophy, *et al.*, *Chemical Warfare Service* (U. S. Army in W. W. II), (R) 417; Leicester, "Chemistry, Chemical Technology, and Scientific Progress," 352; Lockemann, *Story of Chemistry*, (R) 270; Lynch and Vaizey, *Guinness's*

Brewery in the Irish Economy, (R) 273; Partington, *History of Greek Fire and Gunpowder*, (R) 272.
"Chemistry, Chemical Technology, and Scientific Progress," Henry M. Leicester, 352.
Chenault, Lawrence R., (R), Purcell, *Blue Collar Man: Patterns of Dual Allegiance in Industry*, 411; (R), Tew and Henderson, *Studies in Company Finance: A Symposium on the Economic Analysis and Interpretation of British Company Accounts*, 77.
Christian, W. C., (R), Schultz and Whisler, *Management Organization and the Computer*, 193.
Civil Engineering: Calhoun, *American Civil Engineer*, (R) 61; Condit, *American Building Art*, (R) 402; Fitchen, *Construction of Gothic Cathedrals*, (R) 400; Poirier, *Fifteen Wonders of the World*, (R) 269; Ratigan, *Highways over Broad Waters* (David B. Steinman), (R) 204; Schultz and Simons, *Offices in the Sky*, (R) 71; Shelby, "Medieval Masons' Tools: The Level and the Plumb Rule," 127.
Clark, Ronald H., *The Development of the English Traction Engine*, (R) 288.
Classical Mathematics: A Concise History of the Classical Era in Mathematics, Joseph Ehrenfried Hofmann, (R) 86.
Clemen, Rudolf A., Gartmann, *Rings Around the World: Man's Progress from Steam Engine to Stellite*, 37.
Clocks: Eckhardt, *United States Clock and Watch Patents*, (R) 278; Hoopes, *Shop Records of Daniel Burnap, Clockmaker*, (R) 280; Lübke, *Die Uhr*, (R) 49; Needham, *et al.*, *Heavenly Clockwork: The Great Astronomical Clocks of Medieval China*, (R) 50.
Cochrane, Rexmond C. See Brophy, Leo P.
Cole, Charles W., (R), Scoville, *The Persecution of the Huguenots and French Economic Development, 1680-1720*, 174.
Colt: The Man, The Arms, The Company, James Mitchell, (R) 64.
"Commentary: Technology and the History of Ideas," H. Stuart Hughes, 237.
Computers: See also Automation. Schultz and Whisler, *Management Organization and the Computer*, (R) 193.
"Concerning a Unified Concept of Technical Progress: Zvorikine on Drucker's 'Work and Tools,'" A. Zvorikine, 249.
Condit, Carl W., *American Building Art: The Nineteenth Century* (R) 402; (R), Schultz and Simmons, *Offices in the Sky*, 71.
The Construction of Gothic Cathedrals: A Study of Medieval Vault Erection, John Fitchen, (R) 400.
Creativity and Its Cultivation, Harold H. Anderson, (R) 89.
The Crisis We Face: Automation and the Cold War, George Steele and Paul Kircher, (R) 428.
Davison, C. St. C., "Transporting Sixty-Ton Statues in Early Assyria and Egypt," 11.
de Camp, L. Sprague, "Master Gunner Apollonios," 240; (R), Vaughan, *The House of the Double Axe: The Palace at Knossos*, 53.
Dempster, Derek D., *The Tale of the Comet: The Story of the British Jet Airliner*, (R) 291.
The Development of Iron and Steel Technology in China, Joseph Needham, (R) 170.
The Development of the English Traction Engine, Ronald H. Clark, (R) 288.
Diesel, Eugen, Goldbeck, Gustav, and Schildberger, Friedrich, *From Engines to Autos: Five Pioneers in Engine Development and Their Contributions to the Automotive Industry*, (R) 189.
Drucker, Peter F., "Drucker's Comments on Zvorikine," 253; "The Technological Revolution: Notes on the Relationship of Technology, Science, and Culture," 342.
Dryden, Hugh L., "Future Exploration and Utilization of Outer Space," 112.
Due, John F. and Hilton, George W., *The Electric Interurban Railways in America*, (R) 284.
Duncan, Paul and Elbers, Gerald W., *The Scientific Revolution: Challenge and Promise*, (R) 88.

Eckhardt, George H., *United States Clock and Watch Patents, 1790-1890*, (R) 278.
Economics: Cameron, *France and the Economic Development of Europe*, (R) 403; Fourastié, *The Causes of Wealth*, (R) 262; Hoselitz, *Sociological Aspects of Economic Growth*, (R) 264; Robson, *Nationalized Industry and Public Ownership*, (R) 422; Scoville, *Persecution of the Huguenots and French Economic Development*, (R) 174; Tew and Henderson, *Studies in Company Finance*, (R) 77; Youngson, *Possibilities of Economic Progress*, (R) 196.
"Eilmer of Malmesbury, an Eleventh Century Aviator," Lynn White, jr., 97.
Elbers, Gerald W. and Duncan, Paul, *The Scientific Revolution: Challenge and Promise*, (R) 88.
The Electric Interurban Railways in America, George W. Hilton and John F. Due, (R) 284.
Electrical Engineering: Higgins, "Biographical Bibliography of Electrical Engineers and Electrophysicists," 28, 146.
Elihu Thomson: Beloved Scientist, David O. Woodbury, (R) 419.
Emme, Eugene M., *The Impact of Air Power: National Security and World Politics*, (R) 75; (R), Ropp, *War in the Modern World*, 275.
Energy: See also Atomic Energy, Mechanical Engineering. Schurr and Netschert, *Energy in the American Economy*, (R) 285.
Energy in the American Economy, 1850-1975, Sam H. Schurr and Bruce C. Netschert, (R) 285.
"Engineering and Science: A Historical Review and Appraisal," James Kip Finch, 318.
"Engineering and the Scientific Revolution," A. Rupert Hall, 333.
Die Entwicklung der Drehbank bis zum Jahre 1939, Karl Wittmann, (R) 176.
Die Entwicklung des Hochdruckdampfes in Deutschland, Ernst-Otto Jochmann, (R) 178.

Farber, Eduard, (R), Lockemann, *The Story of Chemistry*, 270.
The Farmer's Age: Agriculture, 1815-1860, Paul W. Gates, (R) 185.
Feibleman, James K., "Pure Science, Applied Science, Technology, Engineering: An Attempt at Definitions," 305.
Feldhaus, Franz Maria, *Geschichte de Technischen Zeichnens*, (R) 45.
Ferguson, Eugene S., (R), White, *Yankee from Sweden: The Dream and the Reality in the Days of John Ericsson*, 65.
The Fifteen Wonders of the World, René Poirier, (R) 269.
Finch, James Kip, "Engineering and Science: A Historical Review and Appraisal," 318; *The Story of Engineering*, (R) 33.
Fisher, George J. B. See Brophy, Leo P.
Fitchen, John, *The Construction of Gothic Cathedrals: A Study of Medieval Vault Erection*, (R) 400.

Flaherty, Bernard E., *Psychophysiological Aspects of Space Flight*, (R) 435.
Fogel, Robert William, *The Union Pacific Railroad: A Case in Premature Enterprise*, (R) 405.
Forbes, R. J., *Studies in Early Petroleum History; More Studies in Early Petroleum History*, (R) 55; (R), Finch, *The Story of Engineering*, 33; (R), *Der Ingenieur und Seine Aufgaben in Neuen Wirtschaftsraumen*, 82; (R), Poirer, *The Fifteen Wonders of the World*, 269.
Fourastié, Jean, *The Causes of Wealth*, (R) 262.
France and the Economic Development of Europe, 1800-1914, Rondo E. Cameron, (R) 403.
From Engines to Autos: Five Pioneers in Engine Development and Their Contributions to the Automotive Industry, Eugen Diesel, Gustav Goldbeck, and Friedrich Schildberger, (R) 189.
Fryling, Glenn R., (R), Jochmann, *Entwicklung des Hochdruckdampfes in Deutschland*, 178.
"Future Exploration and Utilization of Outer Space," Hugh L. Dryden, 112.

Gartmann, Heinz, *Rings Around the World: Man's Progress from Steam Engine to Satellite*, (R) 37.
Gates, Paul W., *The Farmer's Age: Agriculture, 1815-1860*, (R) 185.
Geschichte des technischen Zeichens, Fraz Maria Feldhaus, (R) 45.
Ghaswala, S. K., (R), Grunwald and Ronall, *Industrialization in the Middle East*, 79.
Glass: Revi, *Nineteenth Century Glass*, (R) 68.
God Speed the Plow: The Coming of Steam Cultivation to England, Clark C. Spence, (R) 187.
Goldbeck, Gustav. See Diesel, Eugen.
Goodrich, L. Carrington, (R), Needham, Ling, and Price, *Heavenly Clockwork: The Great Astronomical Clocks of Medieval China*, 50.
Grancsay, Stephen V., (R), Oakeshott, *The Archaeology of Weapons: Arms and Armour from Prehistory to the Age of Chivalry*, 171.
Greenleaf, William, *Monopoly on Wheels: Henry Ford and the Selden Automobile Patent*, (R) 289.
Grunwald, Kurt and Ronall, Joachim O., *Industrialization in the Middle East*, (R) 79.
Guest, Robert H., (R), Aitken, *Taylorism at Watertown Arsenal: Scientific Management in Action 1908-1915*, 191.
A Guide to American Trade Catalogs, 1744-1900, Lawrence B. Romaine, (R) 282.
Guinness's Brewery in the Irish Economy, 1759-1876, Patrick Lynch and John Vaizey, (R) 273.
Gullette, George A., (R), Elbers and Duncan, *The Scientific Revolution: Challenge and Promise*, 88; (R), Still, *Science and Education at the Crossroads: A View from the Laboratory*, 300.

Haden, H. J. (R), Revi, *Nineteenth Century Glass: Its Genesis and Development*, 68.
Hall, A. Rupert, "Engineering and the Scientific Revolution," 333; "More on Medieval Pivoted Axles," 17.
Hanford, W. E., (R), Silk, *The Research Revolution*, 298.
Hartenberg, R. S., (R), Feldhaus, *Geschichte des Technischen Zeichens*, 45.
Heald, Morrell, (R), Walsh, *Charleston's Sons of Liberty: A Study of the Artisans, 1763-1789*, 59.
Heavenly Clockwork: The Great Astronomical Clocks of Medieval China, Joseph Needham, Wang Ling, and Derek J. de Solla Price, (R) 50.
Henderson, R. F. and Tew, Brian, *Studies in Company Finance: A Symposium on the Economic Analysis and Interpretation of British Company Accounts*, (R) 77.
Hendrichs, Franz, *Der Weg aus der Tretmühle*, (R) 41.
Herum, Floyd L. and Kohlmeyer, Fred W., "Science and Engineering in Agriculture: A Historical Perspective," 368.
Higgins, Thomas James, "A Biographical Bibliography of Electrical Engineers and Electrophysicists," Part I (Books), 28; Part II (Periodicals), 146.
Highways Over Broad Waters: Life and Times of David B. Steinman, Bridgebuilder, William Ratigan, (R) 204.
Hilton, George W. and Due, John F., *The Electric Interurban Railways in America*, (R) 284.
Historiography of Technology: Burlingame, "Technology: Neglected Clue to Historical Change," 219; Drucker, "The Technological Revolution," 342; Hughes, "Commentary: Technology and the History of Ideas," 237; Joravsky, "History of Technology in Soviet Russia and Marxist Doctrine," 5; Mumford, "History: Neglected Clue to Technological Change," 230; Zvorikine, "Concerning a Unified Concept of Technical Progress," 249; "The History of Technology as a Science and a Branch of Learning," 1.
"History: Neglected Clue to Technological Change," Lewis Mumford, 230.
History of Greek Fire and Gunpowder, J. R. Partington, (R) 272.
A History of Mechanical Inventions, Abbott Payson Usher, (R) 34.
A History of Metallography, Cyril Stanley Smith, (R) 266.
A History of Metals, Leslie Aitchison, (R) 166.
"The History of Technology as a Science and a Branch of Learning," A. Zvorikine, 1.
"The History of Technology in Soviet Russia and Marxist Doctrine," David Joravsky, 5.
History of the Milling Machine: A Study in Technical Development, Robert S. Woodbury, (R) 43.
Hofmann, Joseph Ehrenfried, *Classical Mathematics: A Concise History of the Classical Era in Mathematics*, (R) 86.
Holley, I. B., Jr., (R), Ogorkiewicz, *Armor: A History of Mechanized Forces*, 415.
Hollon, W. Eugene, (R), Mitchell, *Colt: The Man, The Arms, The Company*, 64.
Hoopes, Penrose R., *Shop Records of Daniel Burnap, Clockmaker*, (R) 280.
Hoselitz, Bert F., *The Sociological Aspects of Economic Growth*, (E) 264.
The House of the Double Axe: The Palace at Knossos, Agnes Carr Vaughan, (R) 53.
Houston, Charles O., Jr., (R), Korson, *Black Rock: Mining Folklore of the Pennsylvania Dutch*, 408.
Hubler, Richard G., *Big Eight: A Biography of an Airplane*, (R) 293.
Hughes, H. Stuart, "Commentary: Technology and the History of Ideas," 237.
Hughes, Thomas P., (R), Kramish, *Atomic Energy in the Soviet Union*, 85; (R), Woodbury, *Elihu Thomson: Beloved Scientist*, 419.

The Impact of Air Power: National Security and World Politics, Eugene M. Emme, (R) 75.

Industrial Development: Banani, *Modernization of Iran*, (R) 420; Bredo, *Industrial Estates*, (R) 295; Grunwald and Ronall, *Industrialization in the Middle East*, (R) 99; Michal, *Central Planning in Czechoslovakia* (R) 426; Wheelock, *Nasser's New Egypt*, (R) 201.
Industrial Engineering: Aitken, *Taylorism at Watertown Arsenal*, (R) 191; Bredo, *Industrial Estates*, (R) 295; Brody, *Steelworkers in America*, (R) 409; Purcell, *Blue Collar Man*, (R) 411; Schultz and Whisler, *Management Organization and the Computer*, (R) 198.
Industrial Estates: Tool for Industrialization, William Bredo, (R) 295.
Industrial Relations: See Industrial Engineering.
Industrial Revolution: Smelser, *Social Change in the Industrial Revolution*, (R) 57.
Industrialization in the Middle East, Kurt Grunwald and Joachim O. Ronall, (R) 79.
Der Ingenieur und Seine Aufgaben in Neuen Wirtschaftraumen, (R) 82.
"The Interaction of Science and Practice in the History of Metallurgy," Cyril Stanley Smith, 352.
"Iron and Steel in 1627: The 'Fidelle Ouverture de l'Art de Serrurier' of Mathurin Jousse," Cyril Stanley Smith and Anneliese Sisco, 131.

Jaffe, A. J., (R), Steele and Kircher, *The Crisis We Face: Automation and the Cold War*, 428.
Jochmann, Ernst-Otto, *Die Entwicklung des Hochdruckdampfes in Deutschland*, (R) 178.
Joravsky, David, "The History of Technology in Soviet Russia and Marxist Doctrine," 5.
Jousse, Mathurin, "Fidelle Ouverture de l'Art de Serrurier," 131.

Kaplan, Max, *Leisure in America: A Social Inquiry*, (R) 431.
Kerker, Milton, "Science and the Steam Engine," 381.
Kircher, Paul and Steele, George, *The Crisis We Face: Automation and the Cold War*, (R) 428.
Kohlmeyer, Fred W. and Herum, Floyd L., "Science and Engineering in Agriculture: A Historical Perspective," 368.
Korson, George, *Black Rock: Mining Folklore of the Pennsylvania Dutch*, (R) 408.
Kouwenhoven, John A., (R), Ratigan, *Highways Over Broad Waters: Life and Times of David B. Steinman, Bridgebuilder*, 204.
Kramish, Arnold, *Atomic Energy in the Soviet Union*, (R) 85.
Kurland, Norman D., (R), Kaplan, *Leisure in America: A Social Inquiry*, 431; (R), Pyke, *Slaves Unaware? A Mid-Century View of Applied Science*, 202.

Lampman, L. R., (R), Thornton, *British Shipping*, 73.
Lansdell, Norman, *The Atom and the Energy Revolution*, (R) 83.
Leicester, Henry M., "Chemistry, Chemical Technology, and Scientific Progress," 352; (R), Partington, *History of Greek Fire and Gunpowder*, 272.
Leisure in America: A Social Inquiry, Max Kaplan, (R) 431.
Levey, Martin, (R), Forbes, *Studies in Early Petroleum History; More Studies in Early Petroleum History*, 55.
Levinson, Mark, (R), Hofmann, *Classical Mathematics: A Concise History of the Classical Era in Mathematics*, 86.
Ling, Wang. See Needham, Joseph.
Lockemann, Georg, *The Story of Chemistry*, (R) 270.
Lübke, Anton, *Die Uhr, von der Sonnenuhr zur Atomuhr*, (R) 49.
Lynch, Patrick and Vaizey, John, *Guinness's Brewery in the Irish Economy, 1759-1876*, (R) 273.

Mahler, Harry, (R), Condit, *American Building Art: The Nineteenth Century*, 402.
Management Organization and the Computer, George P. Schultz and Thomas L. Whisler, (R) 198.
"Master Gunner Apollonios," L. Sprague de Camp, 240.
Mathematics: Ehrenfried, *Classical Mathematics*, (R) 86.
Mechnical Engineering: Broehl, *Precision Valley*, (R) 180; Clark, *Development of the English Traction Engine*, (R) 288; Hendrichs, *Der Weg aus der Tretmühle*, (R) 41; Jochmann, *Entwicklung der Hochdruckdampfes in Deutschland*, (R) 178; Kerker, "Science and the Steam Engine," 381; Usher, *History of Mechanical Inventions*, (R) 34; White, *Yankee from Sweden* (John Ericsson), (R) 65; Wittmann, *Entwicklung der Drehbank*, (R) 176; Woodbury, *History of the Milling Machine*, (R) 43.
Medicine and Society in America: 1660-1880, Richard Harrison Shryock, (R) 277.
"Medieval Masons' Tools: The Level and the Plumb Rule," Lonnie R. Shelby, 127.
Mehl, Robert F., (R), Smith, *A History of Metallography*, 266.
Metallurgy: Aitchison, *History of Metals*, (R) 166; Needham, *Development of Iron and Steel Technology in China*, (R) 170; Piaskowski, "Preliminary Report on Investigations of Early Iron Technology in Poland," 245; Smith, *History of Metallography*, (R) 266; Smith, "Interaction of Science and Practice in the History of Metallurgy," 352; Smith and Sisco, "Iron and Steel in 1627: The 'Fidelle Ouverture de l'Art de Serrurier' of Mathurin Jousse," 131.
Michal, Jan M., *Central Planning in Czechoslovakia: Organization for Growth in a Mature Economy*, (R) 426.
Miles, Wyndham D., *et al.* See Brophy, Leo P.
Miller, Genevieve, (R), Shryock, *Medicine and Society in America: 1660-1880*, 277.
Mining: See also Metallurgy. Korson, *Black Rock: Mining Folklore of the Pennsylvania Dutch*, (R) 408.
Mitchell, James, Colt: *The Man, The Arms, The Company*, (R) 64.
The Modernization of Iran, 1921-41, Amin Banani, (R) 420.
Monopoly on Wheels: Henry Ford and the Selden Automobile Patent, William Greenleaf, (R) 289.
"More on Medieval Pivoted Axles," A. Rupert Hall, 17.
More Studies in Early Petroleum History, R. J. Forbes, (R) 55.
Morse, Chandler, (R), Youngson, *Possibilities of Economic Progress*, 196.
Mumford, Lewis, "History: Neglected Clue to Technological Change," 230.
McHugh, Jeanne, (R), Brody, *Steelworkers in America: The Nonunion Era*, 409.

Nasser's New Egypt, Keith Wheelock, (R) 201.
Nationalized Industry and Public Ownership, William A. Robson, (R) 422.
Needham, Joseph, *The Development of Iron and Steel Technology in China*, (R) 170.

Needham, Joseph, Ling, Wang, and Price, Derek J. de Solla, *Heavenly Clockwork: The Great Astronomical Clocks of Medieval China*, (R) 50.
Netschert, Bruce C. and Schurr, Sam H., *Energy in the American Economy, 1850-1975*, (R) 285.
Niles, Alfred S., (R), Dempster, *The Tale of the Comet: The Story of the British Jet Airliner*, 291.
***Nineteenth Century Glass: Its Genesis and Development*, Albert Christian Revi, (R) 68.**

Oakeshott, R. Ewart, *The Archaeology of Weapons: Arms and Armour from Prehistory to the Age of Chivalry*, (R) 171.
***Offices in the Sky*, Earle Schultz and Walter Simmons, (R) 71.**
Ogorkiewicz, Richard M., *Armor: A History of Mechanized Forces*, (R) 415.

Partington, J. R., *History of Greek Fire and Gunpowder*, (R) 272.
Patents: Eckhardt, *United States Clock and Watch Patents*, (R) 278.
***The Persecution of the Huguenots and French Economic Development, 1680-1720*, Warren Chandler Scoville, (R) 174.**
Peterson, Arthur G., *Salt and Salt Shakers*, (R) 71.
Petroleum: Forbes, *Studies in Early Petroleum History; More Studies*, (R) 55.
Piaskowski, J., "Preliminary Report on Investigations of Early Iron Technology in Poland," 245.
Poirier, René, *The Fifteen Wonders of the World*, (R) 269.
***Possibilities of Economic Progress*, A. J. Youngson, (R) 196.**
***Precision Valley: The Machine Tool Companies of Springfield, Vermont*, Wayne G. Broehl, Jr., (R) 180.**
"Preliminary Report on Investigations of Early Iron Technology in Poland," J. Piaskowski, 245.
Price, Derek J. de Solla. See also Needham, Joseph; (R), Lübke, *Die Uhr, von der Sonnenuhr zur Atomuhr*, 49.
***Psychophysiological Aspects of Space Flight*, Bernard E. Flaherty, (R) 433.**
Purcell, Theodore V., *Blue Collar Man: Patterns of Dual Allegiance in Industry*, (R) 411.
"Pure Science, Applied Science, Technology, Engineering: An Attempt at Definitions," James K. Feibleman, 305.
Pyke, Magnus, *Slaves Unaware? A Mid-Century View of Applied Science*, (R) 202.

Rae, John B., "Science and Engineering in the History of Aviation," 391; (R), Diesel, Goldbeck, and Schildberger, *From Engines to Autos: Five Pioneers in Engine Development and Their Contributions to the Automotive Industry*, (R) 189; (R), Greenleaf, *Monopoly on Wheels: Henry Ford and the Selden Automobile Patent*, 289.
Railroads: Fogel, *Union Pacific Railroad*, (R) 405; Due and Hilton, *Electric Interurban Railways in America*, (R) 284.
Rasmussen, Wayne D., *Readings in the History of American Agriculture*, (R) 185; (R), Gates, *The Farmer's Age: Agriculture, 1815-1860*, 185.
Ratigan, William, *Highways Over Broad Waters: Life and Times of David B. Steinman, Bridgebuilder*, (R) 204.
***Readings in the History of American Agriculture*, Wayne D. Rasmussen, (R) 185.**
***The Research Revolution*, Leonard S. Silk, (R) 298.**
Reti, Ladislao, (R), Hendrichs, *Der Weg aus der Tretmühle*, 41.
Revi, Albert Christian, *Nineteenth Century Glass: Its Genesis and Development*, (R) 68.
***Rings Around the World: Man's Progress from Steam Engine to Satellite*, Heinz Gartmann, (R) 37.**
Robson, William A., *Nationalized Industry and Public Ownership*, (R) 422.
Rodnick, David, (R), Banani, *The Modernization of Iran, 1921-41*, 420.
Romaine, Lawrence B., *A Guide to American Trade Catalogs, 1744-1900*, (R) 282.
Ronall, Joachim O. and Grunwald, Kurt, *Industrialization in the Middle East*, (R) 79.
Ropp, Theodore, *War in the Modern World*, (R) 275.
Rossman, Joseph, (R), Peterson, *Salt and Salt Shakers*, 71.

***Salt and Salt Shakers*, Arthur G. Peterson, (R) 71.**
Sanders, Ralph, (R), Emme, *The Impact of Air Power: National Security and World Politics*, 75; (R), Wheelock, *Nasser's New Egypt*, 201.
Scamehorn, Howard L., (R), Hubler, *Big Eight: A Biography of an Airplane*, 293.
Schildberger, Frederich. See Diesel, Eugen.
Schultz, Earle and Simmons, Walter, *Offices in the Sky*, (R) 71.
Schultz, George P. and Whisler, Thomas L., *Management Organization and the Computer*, (R) 193.
Schurr, Sam H. and Netschert, Bruce C., *Energy in the American Economy, 1850-1975*, (R) 285.
***Science and Education at the Crossroads: A View from the Laboratory*, Joseph W. Still, (R) 300.**
Science and Engineering: See No. 4, pp. 305-399.
"Science and Engineering in Agriculture: A Historical Perspective," Fred W. Kohlmeyer and Floyd L. Herum, 368.
"Science and Engineering in the History of Aviation," John B. Rae, 391.
"Science and the Steam Engine," Milton Kerker, 381.
Scientific Revolution: Elbers and Duncan, *The Scientific Revolution*, (R) 88; Hall, "Engineering and the Scientific Revolution," 333.
***The Scientific Revolution: Challenge and Promise*, Geraid W. Elbers and Paul Duncan, (R) 88.**
Scoville, Warren Chandler, *The Persecution of the Huguenots and French Economic Development, 1680-1720*, (R) 174; (R), Cameron, *France and the Economic Development of Europe, 1800-1914*, 403.
Shelby, Lonnie R., "Medieval Mason's Tools: The Level and the Plumb Rrule," 127; (R), Fitchen, *The Construction of Gothic Cathedrals: A Study of Medieval Vault Erection*, 400.
***Shop Records of Daniel Burnap, Clockmaker*, Penrose R. Hoopes, (R) 280.**
Shryock, Richard Harrison, *Medicine and Society in America: 1660-1880*, (R) 277.
Silk, Leonard S., *The Research Revolution*, (R) 298.
Simmons, Walter and Schultz, Earle, *Offices in the Sky*, (R) 71.
Sisco, Anneliese and Smith, Cyril Stanley, "Iron and Steel in 1627: The 'Fidelle Ouverture de l'Art de Serrurier' of Mathurin Jousse," 131.

Skolnik, Herman, (R), Brophy and Fisher, *U. S. Army in World War II. The Technical Services. The Chemical Warfare Service: Organizing for War*. Brophy, Miles, and Cochrane, *From Laboratory to Field*, (R) 417.
Slaves Unaware? A Mid-Century View of Applied Science, Magnus Pyke, (R) 202.
Smelser, Neil J., *Social Change in the Industrial Revolution: An Application of Theory to the British Cotton Industry, 1770-1840*, 57.
Smith, Cyril Stanley, "The Interaction of Science and Practice in the History of Metallurgy," 352; *A History of Metallography*, (R) 266; (R), Aitchison, *A History of Metals*, 166.
Smith, Cyril Stanley and Sisco, Anneliese, "Iron and Steel in 1627: The 'Fidelle Ouverture de l'Art de Serrurier' of Mathurin Jousse," 131.
Smith, Thomas M., (R), Usher, *A History of Mechanical Inventions*, 34.
Social Change: Hoselitz, *Sociological Aspects of Economic Growth*, (R) 264; Kaplan, *Leisure in America*, (R) 431; Pyke, *Slaves Unaware?*, (R) 202; Smelser, *Social Change in the Industrial Revolution*, (R) 57.
Social Change in the Industrial Revolution: An Application of Theory to the British Cotton Industry, 1770-1840, Neil J. Smelser, (R) 57.
The Sociological Aspects of Economic Growth, Bert F. Hoselitz, (R) 264.
Spence, Clark C., *God Speed the Plow: The Coming of Steam Cultivation to England*, (R) 187.
Steele, George and Kircher, Paul, *The Crisis We Face: Automation and the Cold War*, (R) 428.
Steelworkers in America: The Nonunion Era, David Brody, (R) 409.
David B. Steinman: "An Appreciation of David Bernard Steinman (1886-1960)," Sarah Ruth Watson, 23; Ratigan, *Highways over Broad Waters: Life and Times of David B. Steinman, Bridgebuilder*, (R) 204.
Steinmetz, Rollin G., *Vanishing Crafts and Their Craftsmen*, (R) 407.
Still, Joseph W., *Science and Education at the Crossroads: A View from the Laboratory*, (R) 300.
The Story of Chemistry, Georg Lockemann, (R), 270.
The Story of Engineering, James Kip Finch, (R) 33.
Studies in Company Finance: A Symposium on the Economic Analysis and Interpretation of British Company Accounts, Brian Tew and R. F. Henderson, (R) 77.
Studies in Early Petroleum History, R. J. Forbes, (R) 55.

The Tale of the Comet: The Story of the British Jet Airliner, Derek D. Dempster, (R) 291.
Taylorism at Watertown Arsenal: Scientific Management in Action 1908-1915, Hugh G. J. Aitken, (R) 191.
"Technoculture," Henry G. Burger, 260.
"The Technological Revolution: Notes on the Relationship of Technology, Science, and Culture," Peter F. Drucker, 342.
"Technology: Neglected Clue to Historical Change," Roger Burlingame, 219.
Tew, Brian and Henderson, R. F., *Studies in Company Finance: A Symposium on the Economic Analysis and Interpretation of British Company Accounts*, (R) 77.
Thompson, B. C., (R), Lansdell, *The Atom and the Energy Revolution*, 83; Schurr and Netschert, *Energy in the American Economy, 1850-1975*, 285.
Thornton, R. H., *British Shipping*, (R) 73.
Transportation: See also Railroads. Davison, "Transporting Sixty-Ton Statues in Early Assyria and Egypt," 11; Gartmann, *Rings Around the World*, (R) 37; Hall, "More on Medieval Pivoted Axles," 17; Thornton, *British Shipping*, (R) 73.
"Transporting Sixty-Ton Statues in Early Assyria and Egypt," C. St. C. Davison, 11.

Die Uhr, von der Sonnenuhr zur Atomuhr, Anton Lübke, (R) 49.
The Union Pacific Railroad: A Case in Premature Enterprise, Robert William Fogel, (R) 405.
U. S. Army in World War II. The Technical Services. The Chemical Warfare Service: Organizing for War, Leo P. Brophy and George J. B. Fisher. *From Laboratory to Field*, Leo P. Brophy, Wyndham D. Miles, and Rexmond C. Cochrane, (R) 417.
United States Clock and Watch Patents, 1790-1890, George H. Eckhardt, (R) 278.
Usher, Abbott Payson, *A History of Mechanical Inventions*, (R) 34; (R), Woodbury, *History of the Milling Machine: A Study in Technical Development*, 43.

Vaizey, John and Lynch, Patrick, *Guinness's Brewery in the Irish Economy, 1759-1876*, (R) 273.
Vanishing Crafts and Their Craftsmen, Rollin G. Steinmetz, (R) 407.
Vaughan, Agnes Carr, *The House of the Double Axe: The Palace at Knossos*, (R) 53.

Walsh, Richard, *Charleston's Sons of Liberty: A Study of the Artisans, 1763-1789*, 59.
War in the Modern World, Theodore Ropp, (R) 275.
Watson, Sarah Ruth, "An Appreciation of David Bernard Steinman (1886-1960)," 23; (R), Calhoun, *The American Civil Engineer*, 61.
Weapons: See also Armor. de Camp, "Master Gunner Apollonios," 240; Mitchell, *Colt*, (R) 64; Partington, *History of Greek Fire and Gunpowder*, (R) 272.
Der Weg aus der Tretmühle, Franz Hendrichs, (R) 41.
Wheelock, Keith, *Nasser's New Egypt*, (R) 201.
Whisler, Thomas L. and Schultz, George P., *Management Organization and The Computer*, (R) 193.
White, Lynn, jr., "Eilmer of Malmesbury, an Eleventh Century Aviator," 97.
White, Ruth, *Yankee From Sweden: The Dream and the Reality in the Days of John Ericsson*, (R) 65.
Wik, Reynold M., (R), Spence, *God Speed the Plow: The Coming of Steam Cultivation to England*, 187.
"Williams Reviews the Review Issue," Trevor I. Williams, 254.
Williams, Trevor I., "Williams Reviews the Review Issue," 254; (R), Lynch and Vaizey, *Guinness's Brewery in the Irish Economy, 1759-1876*, 273.
Williamson, Harold F., (R), Hilton and Due, *The Electric Interurban Railways in America*, 284.
Wisnewsky, Edward, (R), Robson, *Nationalized Industry and Public Ownership*, 422.
Wittmann, Karl, *Die Entwicklung der Drehbank bis zum Jahre 1939*, (R) 176.

Woodbury, David O., *Elihu Thomson: Beloved Scientist*, (R) 419.
Woodbury, Robert S., *History of the Milling Machine: A Study in Technical Development*, (R) 43; Broehl, *Precision Valley: The Machine Tool Companies of Springfield, Vermont*, 180; (R), Wittmann, *Die Entwicklung der Drehbank bis zum Jahre 1939*, 176.

Yankee from Sweden: The Dream and the Reality in the Days of John Ericsson, Ruth White, (R) 65.

Youngson, A. J., *Possibilities of Economic Progress*, (R) 196.

Zvorikine, A., "Concerning a Unified Concept of Technical Progress: Zvorikine on Drucker's 'Work and Tools,'" 249; "The History of Technology as a Science and a Branch of Learning," 1.

3078